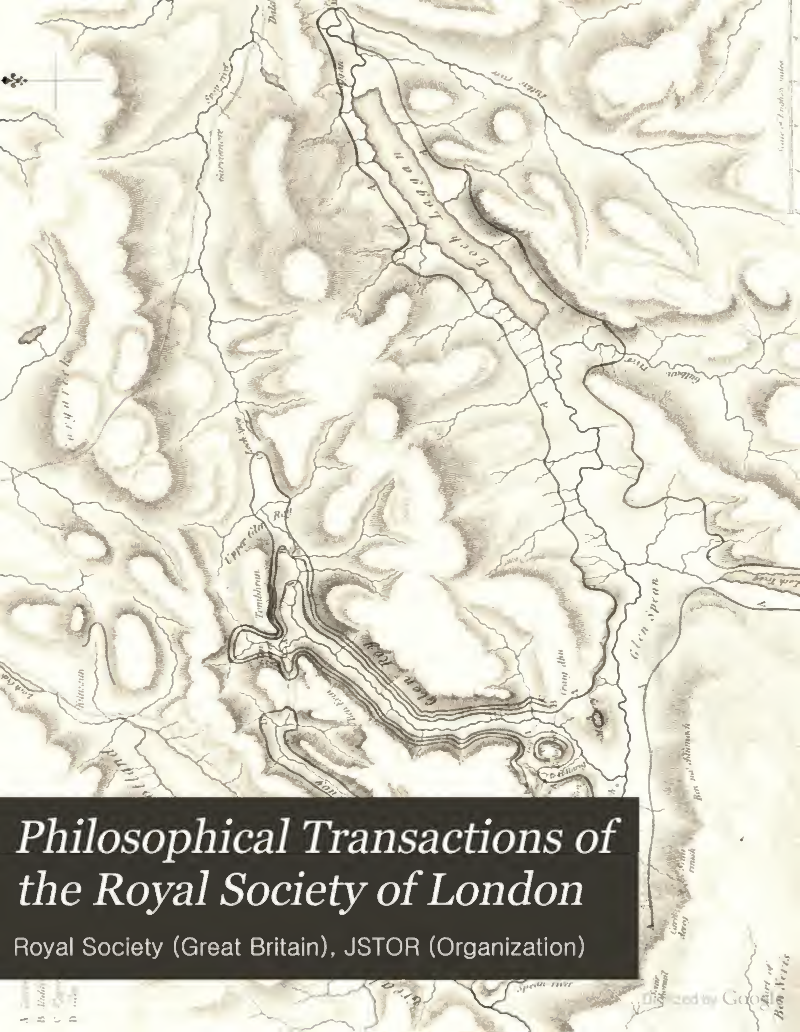

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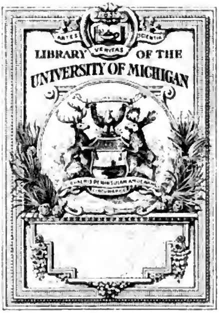
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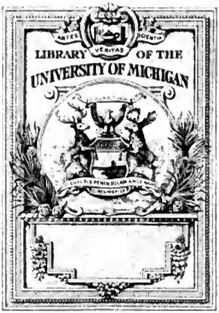
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PHILOSOPHICAL
TRANSACTIONS

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXXXIX.

PART I.

LONDON:
PRINTED BY RICHARD AND JOHN E. TAYLOR, RED LION COURT, FLEET STREET.
MDCCCXXXIX.

ADVERTISEMENT.

The Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions,* take this opportunity to acquaint the Public, that it fully appears, as well from the Council-books and Journals of the Society, as from repeated declarations which have been made in several former *Transactions,* that the printing of them was always, from time to time, the single act of the respective Secretaries till the Forty-seventh Volume; the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgement of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they received them, are to be considered in no other light than as a matter of civility, in return for the respect shown to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report and even to certify in the public newspapers, that they have met with the highest applause and approbation. And therefore it is hoped that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

A List of Public Institutions and Individuals, entitled to receive a copy of the Philosophical Transactions of each year, on making application for the same directly or through their respective agents, within five years of the date of publication.

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The Observatory at Edinburgh.

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The Entomological Society of France.
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The Jardin des Plantes, Paris.

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The Cæsarean Academy of Naturalists at Bonn.
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The Italian Society of Sciences at Modena.
The Royal Academy of Sciences at Turin.

Switzerland.

The Société de Phys. et d'Hist. Nat. at Geneva.

Belgium.

The Royal Academy of Sciences at Brussels.

Netherlands.

The Royal Institute of Amsterdam.

Spain.

The Royal Observatory at Cadiz.

Portugal.

The Royal Academy of Sciences at Lisbon.

Prussia.

The Royal Academy of Sciences at Berlin.

Russia.

The Imperial Academy of Sciences at St. Petersburgh.

Sweden and Norway.

The Royal Academy of Sciences at Stockholm.
The Royal Society of Sciences at Drontheim.

United States.

The American Philosophical Society at Philadelphia.
The New York Philosophical Society.
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The *fifty* Foreign Members of the Royal Society.

A List of Public Institutions and Individuals, entitled to receive a copy of the Astronomical Observations made at the Royal Observatory at Greenwich, on making application for the same directly or through their respective agents, within two years of the date of publication.

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The University of Dublin.
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The Observatory at Armagh.
The Observatory at the Cape of Good Hope.
The Observatory at Paramatta.
The Observatory at Madras.
The Royal Institution of Great Britain.
The Royal Society, Edinburgh.
The Astronomical Institution, Edinburgh.
The President of the Royal Society.
The Lowndes's Professor of Astronomy, Cambridge.
The Plumian Professor of Astronomy, Cambridge.
Francis Baily, Esq. V.P. and Treas. R.S.
Thomas Henderson, Esq. of Edinburgh.
John William Lubbock, Esq.
Captain W. H. Smyth, R.N. of Bedford.
Sir James South, Observatory, Kensington.

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The Royal Academy of Sciences at Berlin.
The Royal Academy of Sciences at Paris.
The Imperial Academy of Sciences at St. Petersburgh.
The Royal Academy of Sciences at Stockholm.
The Royal Society of Sciences at Upsal.
The Board of Longitude of France.
The University of Göttingen.
The University of Leyden.
The Academy of Bologna.
The American Academy of Sciences at Boston.
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The Library of Harvard College.
The Observatory at Helsingfors.
The Observatory at Altona.
The Observatory at Berlin.
The Observatory at Breslau.
The Observatory at Brussels.
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The Observatory at Königsberg.
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The Observatory at Marseilles.
The Observatory at Milan.
The Observatory at Munich.
The Observatory at Palermo.
The Observatory at Paris.
The Observatory at Seeberg.
The Observatory at Vienna.
The Observatory at Tubingen.
The Observatory at Wilna.
Professor Bessel, of Königsberg.
Dr. William Olbers, of Bremen.
The Dépôt de la Marine, Paris.
The Bowden College, United States.
The Waterville College, United States.

ROYAL MEDALS.

HER MAJESTY QUEEN VICTORIA, in restoring the Foundation of the Royal Medals, has been graciously pleased to approve of the following regulations for the award of them:

That the Royal Medals be given for such papers only as have been presented to the Royal Society, and inserted in their Transactions.

That the triennial Cycle of subjects be the same as that hitherto in operation: viz.

1. Astronomy; Physiology, including the Natural History of Organized Beings.
2. Physics; Geology and Mineralogy.
3. Mathematics; Chemistry.

That, in case no paper, coming within these stipulations, should be considered deserving of the Royal Medal, in any given year, the Council have the power of awarding such Medal to the author of any other paper on either of the several subjects forming the Cycle, that may have been presented to the Society and inserted in their Transactions; preference being given to the subjects of the year immediately preceding: the award being, in such case, subject to the approbation of Her Majesty.

The Council propose to give one of the Royal Medals in the year 1840 for the most important unpublished paper in Physics, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1837, and prior to the termination of the Session in June 1840.

The Council propose also to give one of the Royal Medals in the year 1840 for the most important unpublished paper in Geology or Mineralogy, communicated to the

Royal Society for insertion in their Transactions after the termination of the Session in June 1837, and prior to the termination of the Session in June 1840.

The Council propose to give one of the Royal Medals in the year 1841 for the most important unpublished paper in Mathematics, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1838, and prior to the termination of the Session in June 1841.

The Council propose also to give one of the Royal Medals in the year 1841 for the most important unpublished paper in Chemistry, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1838, and prior to the termination of the Session in June 1841.

The Council propose to give one of the Royal Medals in the year 1842 for the most important unpublished paper in Astronomy, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1839, and prior to the termination of the Session in June 1842.

The Council propose also to give one of the Royal Medals in the year 1842 for the most important unpublished paper in Physiology, including the Natural History of Organized Beings, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1839, and prior to the termination of the Session in June 1842.

ADJUDICATION of the MEDALS of the ROYAL SOCIETY for the year 1838 by HIS ROYAL HIGHNESS the PRESIDENT and COUNCIL.

The RUMFORD MEDAL to JAMES DAVID FORBES, Esq., F.R.S., for his "Experiments on the Polarization of Heat," published in the Transactions of the Royal Society of Edinburgh.

A COPLEY MEDAL to Professor K. F. GAUSS, Forn. Memb. R.S., for his "Inventions and Mathematical Researches on Magnetism."

Another COPLEY MEDAL to MICHAEL FARADAY, ESQ., D.C.L., F.R.S., for his "Researches on Specific Electrical Induction."

The ROYAL MEDAL, in the department of Chemistry, to THOMAS GRAHAM, Esq., M.A., F.R.S., for his paper entitled "Inquiries respecting the Constitution of Salts, of Oxalates, Nitrates, Phosphates, Sulphates, and Chlorides," published in the Philosophical Transactions for 1836.

The ROYAL MEDAL, in the department of Mathematics, to WILLIAM HENRY FOX TALBOT, Esq., F.R.S., for his papers entitled "Researches in the Integral Calculus," published in the Philosophical Transactions for 1836 and 1837.

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PHILOSOPHICAL TRANSACTIONS.

I. *Experimental Researches in Electricity.—Fifteenth Series.*

By Michael Faraday, *Esq., D.C.L. F.R.S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, Gottingen, Modena, Stockholm, &c. &c.*

Received November 15,—Read December 6, 1838.

§ 23. *Notice of the character and direction of the electric force of the Gymnotus.*

1749. WONDERFUL as are the laws and phenomena of electricity when made evident to us in inorganic or dead matter, their interest can bear scarcely any comparison with that which attaches to the same force when connected with the nervous system and with life; and though the obscurity which for the present surrounds the subject may for the time also veil its importance, every advance in our knowledge of this mighty power in relation to inert things, helps to dissipate that obscurity, and to set forth more prominently the surpassing interest of this very high branch of Physical Philosophy. We are indeed but upon the threshold of what we may, without presumption, believe man is permitted to know of this matter; and the many eminent philosophers who have assisted in making this subject known, have, as is very evident in their writings, felt up to the latest moment that such is the case.

1750. The existence of animals able to give the same concussion to the living system as the electrical machine, the voltaic battery, and the thunder storm, being with their habits made known to us by Richer, S'Gravesende, Firmin, Walsh, Humboldt, &c. &c., it became of growing importance to identify the living power which they possess, with that which man can call into action from inert matter, and by him named electricity (265. 351.). With the *Torpedo* this has been done to perfection, and the direction of the current of force determined by the united and successive labours of Walsh[*], Cavendish[†], Galvani[‡], Gardini[§], Humboldt and Gay-

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[*] Philosophical Transactions, 1773, p. 461.

[†] Ibid. 1776, p. 196.

[‡] Aldini's Essai sur la Galvanism, ii. 61.

[§] De Electrici ignis Natura, §. 71. Mantua, 1792.

Lussac[*], Todd[†], Sir Humphry Davy[‡], Dr. Davy[§], Becquerel[||], and Matteucci[¶].

1751. The Gymnotus has also been experimented with for the same purpose, and the investigations of Williamson[**], Garden[††], Humboldt[‡‡], Fahlberg[§§] and Guisan[||||], have gone very far in showing the identity of the electric force in this animal with the electricity excited by ordinary means; and the two latter philosophers have even obtained the spark.

1752. As an animal fitted for the further investigation of this refined branch of science, the Gymnotus seems, in certain respects, better adapted than the Torpedo, especially (as Humboldt has remarked) in its power of bearing confinement, and capability of being preserved alive and in health for a long period. A Gymnotus has been kept for several months in activity, whereas Dr. Davy could not preserve Torpedos above twelve or fifteen days; and Matteucci was not able out of 116 such fish to keep one living above three days, though every circumstance favourable to their preservation was attended to[¶¶]. To obtain Gymnoti has therefore been a matter of consequence; and being stimulated, as much as I was honoured, by very kind communications from Baron Humboldt, I in the year 1835 applied to the Colonial Office, where I was promised every assistance in procuring some of these fishes, and continually expect to receive either news of them or the animals themselves.

1753. Since that time Sir Everard Home has also moved a friend to send some Gymnoti over, which are to be consigned to His Royal Highness our late President: and other gentlemen are also engaged in the same work. This spirit induces me to insert in the present communication that part of the letter from Baron Humboldt which I received as an answer to my inquiry of how they were best to be conveyed across the Atlantic. He says, "The Gymnotus, which is common in the Llanos de Caracas (near Calabozo), in all the small rivers which flow into the Orinoco, in English, French or Dutch Guiana, is not of difficult transportation. We lost them so soon at Paris because they were too much fatigued (by experiments) immediately after their arrival. MM. Norderling and Fahlberg retained them alive at Paris above four months. I would advise that they be transported from Surinam (from Essequibo, Demerara, Cayenne) in summer, for the Gymnotus in its native country lives in water of 25° centigrade (or 77° Fahr.). Some are five feet in length, but I would advise that such as are about twenty-seven or twenty-eight inches in length be chosen. Their power varies with their food, and their state of rest. Having but a small stomach they eat little and often, their food being cooked meat, *not salted*, small fish, or even bread. Trial should be made of their strength and the fit kind of nourishment before they are shipped, and those fish only selected already accustomed to their prison. I retained them in a box or trough about four feet long, and sixteen inches wide and deep. The water must be *fresh*, and be changed every three or four days: the fish must not be prevented from coming to the surface, for they like to swallow air. A net should be put over and round the trough, for the Gymnotus often springs out of the water. These are all the directions that I can give you. It is, however, *important* that the animal should not be tormented or fatigued, for it becomes exhausted by frequent electric explosions. Several Gymnoti may be retained in the same trough."

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[*] Annales de Chimie, xiv. 15.

[†] Philosophical Transactions, 1816, p. 120.

[‡] Ibid. 1829, p. 15.

[§] Ibid. 1832, p. 259; and 1834, p. 531.

[||] Traité de l'Électricité, iv. 264.

[¶] Bibliothèque Universelle, 1837, tom. xii. 163.

[**] Philosophical Transactions, 1775, p. 94.

[††] Ibid. 1775, p. 102.

[‡‡] Personal Narrative, chap. xvii.

[§§] Swedish Transactions, 1801, pp. 122. 156.

[||||] De Gymnoto Electrico. Tubingen, 1819.

[¶¶] Bibliothèque Universelle, 1837, xii. p. 174.

1754. A Gymnotus has lately been brought to this country by Mr. **Porter**, and purchased by the proprietors of the Gallery in Adelaide Street: they immediately most liberally offered me the liberty of experimenting with the fish for scientific purposes; they placed it for the time exclusively at my disposal, that (in accordance with **Humboldt**'s directions (1753.)) its powers might not be impaired: only desiring me to have a regard for its life and health. I was not slow to take advantage of their wish to forward the interests of science, and with many thanks accepted their offer. With this Gymnotus, having the kind assistance of Mr. **Bradley** of the Gallery, Mr. **Gassiot**, and occasionally other gentlemen, as Professors **Daniell**, **Owen** and **Wheatstone**, I have obtained every proof of the identity of its power with common electricity (265. 351, &c.). All of these had been obtained before with the Torpedo (1750.), and some, as the shock, circuit, and spark (1751.), with the Gymnotus; but still I think a brief account of the results will be acceptable to the Royal Society, and I give them as necessary preliminary experiments to the investigations which we may hope to institute when the expected supply of animals arrives (1752.).

1755. The fish is forty inches long. It was caught about March 1838; was brought to the Gallery on the 15th of August, but did not feed from the time of its capture up to the 19th of October. From the 24th of August Mr. **Bradley** nightly put some blood into the water, which was changed for fresh water next morning, and in this way the animal perhaps obtained some nourishment. On the 19th of October it killed and eat four small fish; since then the blood has been discontinued, and the animal has been improving ever since, consuming upon an average one fish daily*.

1756. I first experimented with it on the 3rd of September, when it was apparently languid, but gave strong shocks when the hands were favourably disposed on the body (1760. 1773, &c.). The experiments were made on four different days, allowing periods of rest from a month to a week between each. His health seemed to improve continually, and it was during this period, between the third and fourth days of experiment, that he began to eat.

1757. Beside the hands two kinds of collectors were used. The one sort consisted each of a copper rod fifteen inches long, having a copper disc one inch and a half in diameter brazed to one extremity, and a copper cylinder to serve as a handle, with large contact to the hand, fixed to the other, the rod from the disc upwards being well covered with a thick caoutchouc tube to insulate that part from the water. By these the states of particular parts of the fish whilst in the water could be ascertained.

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* The fish eaten were gudgeons, carp, and perch.

1758. The other kind of collectors were intended to meet the difficulty presented by the complete immersion of the fish in water; for even when obtaining the spark itself I did not think myself justified in asking for the removal of the animal into air. A plate of copper eight inches long by two inches and a half wide, was bent into a saddle shape, that it might pass over the fish, and inclose a certain extent of the back and sides, and a thick copper wire was brazed to it, to convey the electric force to the experimental apparatus; a jacket of sheet caoutchouc was put over the saddle, the edges projecting at the bottom and the ends; the ends were made to converge so as to fit in some degree the body of the fish, and the bottom edges were made to spring against any horizontal surface on which the saddles were placed. The part of the wire liable to be in the water was covered with caoutchouc.

1759. These conductors being put over the fish, collected power sufficient to produce many electric effects; but when, as in obtaining the spark, every possible advantage was needful, then glass plates were placed at the bottom of the water, and the fish being over them, the conductors were put over it until the lower caoutchouc edges rested on the glass, so that the part of the animal within the caoutchouc was thus almost as well insulated as if the Gymnotus had been in the air.

1760. *Shock.* The shock of this animal was very powerful when the hands were placed in a favourable position, i. e. one on the body near the head, and the other near the tail; the nearer the hands were together within certain limits the less powerful was the shock. The disc conductors (1757.) conveyed the shock very well when the hands were wetted and applied in close contact with the cylindrical handles; but scarcely at all if the handles were held in the dry hands in an ordinary way.

1761. *Galvanometer.* Using the saddle conductors (1758.) applied to the anterior and posterior parts of the Gymnotus, a galvanometer was readily affected. It was not particularly delicate; for zinc and platina plates on the upper and lower surface of the tongue did not cause a permanent deflection of more than 25°; yet when the fish gave a powerful discharge the deflection was as much as 30°, and in one case even 40°. The deflection was constantly in a given direction, the electric current being always from the anterior parts of the animal through the galvanometer wire to the posterior parts. The former were therefore for the time externally positive, and the latter negative.

1762. *Making a magnet.* When a little helix containing twenty-two feet of silked wire wound on a quill was put into the circuit, and an annealed steel needle placed in the helix, the needle became a magnet, and the direction of its polarity in every case indicated a current from the anterior to the posterior parts of the Gymnotus through the conductors used.

1763. *Chemical decomposition.* Polar decomposition of a solution of iodide of potassium was easily obtained. Three or four folds of paper moistened in the solution

(322.) were placed between a platina plate and the end of a wire also of platina, these being respectively connected with the two saddle conductors (1758.). Whenever the wire was in conjunction with the conductor at the fore part of the Gymnotus, iodine appeared at its extremity; but when connected with the other conductor none was evolved at the place on the paper where it before appeared. So that here again the direction of the current proved to be the same as that given by the former tests.

1764. By this test I compared the middle part of the fish with other portions before and behind it, and found that the conductor A, which being applied to the middle was negative to the conductor B applied to the anterior parts, was, on the contrary, positive to it when B was applied to places near the tail. So that within certain limits the condition of the fish externally at the time of the shock appears to be such, that any given part is negative to other parts anterior to it, and positive to such as are behind it.

1765. *Evolution of heat.* Using a HARRIS's thermo-electrometer belonging to Mr. GASSIOT, we thought we were able in one case, namely, that when the deflection of the galvanometer was 40° (1761.), to observe a feeble elevation of temperature. I was not observing the instrument myself, and one of those who at first believed they saw the effect now doubts the result*.

1766. *Spark.* The electric spark was obtained thus. A good magneto-electric coil, with a core of soft iron wire, had one extremity made fast to the end of one of the saddle collectors (1758.), and the other fixed to a new steel file; another file was made fast to the end of the other collector. One person then rubbed the point of one of these files over the face of the other, whilst another person put the collectors over the fish, and endeavoured to excite it to action. By the friction of the files contact was made and broken very frequently; and the object was to catch the moment of the current through the wire and helix, and by breaking contact *during the current* to make the electricity sensible as a spark.

1767. The spark was obtained four times, and nearly all who were present saw it. That it was not due to the mere attrition of the two piles was shown by its not occurring when the files were rubbed together, independently of the animal. Since then I have substituted for the lower file a revolving steel plate, cut file fashion on its face, and for the upper file wires of iron, copper and silver, with all of which the spark was obtained†.

1768. Such were the general electric phenomena obtained from this Gymnotus whilst living and active in his native element. On several occasions many of them were obtained together; thus a magnet was made, the galvanometer deflected, and perhaps a wire heated, by one single discharge of the electric force of the animal.

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* In more recent experiments of the same kind we could not obtain the effect.

† At a later meeting, at which attempts were made to cause the attraction of gold leaves, the spark was obtained directly between fixed surfaces, the inductive coil (1766.) being removed, and only short wires (by comparison) employed.

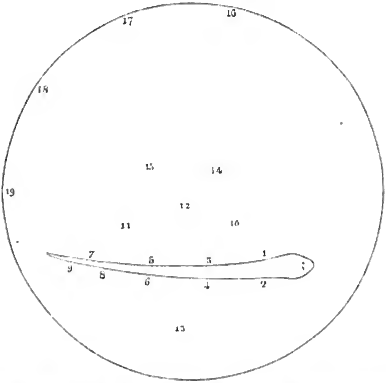
1769. I think a few further but brief details of experiments relating to the quantity and disposition of the electricity in and about this wonderful animal will not be out of place in this short account of its powers.

1770. When the shock is strong, it is like that of a large Leyden battery charged to a low degree, or that of a good voltaic battery of perhaps one hundred or more pair of plates, of which the circuit is completed for a moment only. I endeavoured to form some idea of the *quantity* of electricity by connecting a large Leyden battery (291.) with two brass balls, above three inches in diameter, placed seven inches apart in a tub of water, so that they might represent the parts of the Gymnotus to which the collectors had been applied; but to lower the intensity of the discharge, eight inches in length of six-fold thick wetted string were interposed elsewhere in the circuit, this being found necessary to prevent the easy occurrence of the spark at the ends of the collectors (1758.), when they were applied in the water near to the balls, as they had been before to the fish. Being thus arranged, when the battery was strongly charged and discharged, and the hands put into the water near the balls, a shock was felt, much resembling that from the fish; and though the experiments have no pretension to accuracy, yet as the tension could be in some degree imitated by reference to the more or less ready production of a spark, and after that the shock be used to indicate whether the quantity was about the same, I think we may conclude that a single medium discharge of the fish is at least equal to the electricity of a Leyden battery of fifteen jars, containing 3500 square inches of glass coated on both sides, charged to its highest degree (291.). This conclusion respecting the great quantity of electricity in a single Gymnotus shock, is in perfect accordance with the degree of deflection which it can produce in a galvanometer needle (367. 860. 1761.), and also with the amount of chemical decomposition produced (374. 860. 1763.) in the electrolyzing experiments.

1771. Great as is the force in a single discharge, the Gymnotus, as **Humboldt** describes, and as I have frequently experienced, gives a double and even a triple shock; and this capability of immediately repeating the effect with scarcely a sensible interval of time, is very important in the considerations which must arise hereafter respecting the origin and excitement of the power in the animal. **Walsh**, **Humboldt**, **Gay Lussac**, and **Matteucci** have remarked the same thing of the Torpedo, but in a far more striking degree.

1772. As, at the moment when the fish wills the shock, the anterior parts are positive and the posterior parts negative, it may be concluded that there is a current from the former to the latter through every part of the water which surrounds the animal, to a considerable distance from its body. The shock which is felt, therefore, when the hands are in the most favourable position, is the effect of a very small portion only of the electricity which the animal discharges at the moment, by far the largest portion passing through the surrounding water. This enormous external current must be accompanied by some effect within the fish *equivalent* to a current, the direction of which is from the tail towards the head, and equal to the sum of *all these external forces*. Whether the process of evolving or exciting the electricity within the fish includes the production of this internal current (which need not of necessity be as quick and momentary as the external one), we cannot at present say; but at the time of the shock the animal does not apparently feel the electric sensation which he causes in those around him.

1773. By the help of the accompanying diagram I will state a few experimental results which illustrate the current around the fish, and show the cause of the difference in character of the shock occasioned by the various ways in which the person is connected with the animal, or his position altered with respect to it. The large circle represents the tub in which the animal is confined; its diameter is forty-six inches, and the depth of water in it three inches and a half; it is supported on dry wooden legs. The figures represent the places where the hands or the disc conductors (1757.) were applied, and where they are close to the figure of the animal, it implies that contact with the fish was made. I will designate different persons by A, B, C, &c., A being the person who excited the fish to action.



1774. When one hand was in the water the shock was felt in that hand only, whatever part of the fish it was applied to; it was not very strong, and was only in the part immersed in the water. When the hand and part of the arm was in, the shock was felt in all the parts immersed.

1775. When *both* hands were in the water at the *same* part of the fish, still the shock was comparatively weak, and only in the parts immersed. If the hands were on opposite sides, as at 1, 2, or at 3, 4, or 5, 6, or if one was above and the other below at the same part, the effect was the same. When the disc collectors were used in these positions no effect was felt by the person holding them, (and this corresponds with the observation of GAY-LUSSAC on Torpedos*,) whilst other persons, with both hands in at a distance from the fish, felt considerable shocks.

1776. When both hands or the disc collectors were applied at places separated by a part of the length of the animal, as at 1, 3, or 4, 6, or 3, 6, then strong shocks extending up the arms, and even to the breast of the experimenter, occurred, though another person with a single hand in at any of these places, felt comparatively little. The shock could be obtained at parts very near the tail, as at 8, 9. I think it was strongest at about 1 and 8. As the hands were brought nearer together the effect diminished, until being in the same cross plane, it was, as before described, only sensible in the parts immersed (1775.).

1777. B placed his hands at 10, 11, at least four inches from the fish, whilst A touched the animal with a glass rod to excite it to action; B quickly received a powerful shock. In another experiment of a similar kind, as respects the non-necessity of touching the fish, several persons received shocks independently of each other; thus A was at 4, 6; B at 10, 11; C at 16, 17; and D at 18, 19; all were shocked at once, A and B very strongly, C and D feebly. It is very useful, whilst experimenting with the galvanometer or other instrumental arrangements, for one person to keep his hands in the water at a moderate distance from the animal, that he may know and give information when a discharge has taken place.

1778. When B had both hands at 10, 11, or at 14, 15, whilst A had but one hand at 1, or 3, or 6, the former felt a strong shock, whilst the latter had but a weak one, though in contact with the fish. Or if A had both hands in at 1, 2, or 3, 4, or 5, 6, the effect was the same.

1779. If A had the hands at 3, 5, B at 14, 15, and C at 16, 17, A received the most powerful shock, B the next powerful, and C the feeblest.

1780. When A excited the Gymnotus by his hands at 8, 9, whilst B was at 10, 11, the latter had a much stronger shock than the former, though the former touched and excited the animal.

1781. A excited the fish by one hand at 3, whilst B had both hands at 10, 11 (or along), and C had the hands at 12, 13 (or across); A had the pricking shock in the immersed hand only (1774.); B had a strong shock up the arms; C felt but a slight effect in the immersed parts.

1782. The experiments I have just described are of such a nature as to require many repetitions before the general results drawn from them can be considered as established; nor do I pretend to say that they are anything more than indications of the direction of the force. It is not at all impossible that the fish may have the power of throwing each of its four electric organs separately into action, and so to a certain degree direct the shock, i. e. he may have the capability of causing the electric current to emanate from one side, and at the same time bring the other side of his body into such a condition, that it shall be as a non-conductor in that direction. But I think the appearances and results are such as to forbid the supposition, that he has any control over the direction of the currents after they have entered the fluid and substances around him.

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* Annales de Chimie, xiv. p. 18.

1783. The statements also have reference to the fish when in a straight form; if it assume a bent shape, then the lines of force around it vary in their intensity in a manner that may be anticipated theoretically. Thus if the hands were applied at 1, 7, a feebler shock in the arms would be expected if the animal were curved with that side inwards, than if it were straight, because the distance between the parts would be diminished, and the intervening water therefore conduct more of the force. But with respect to the parts *immersed*, or to animals, as fish *in the water* between 1 and 7, they would be more powerfully, instead of less powerfully, shocked.

1784. It is evident from all the experiments, as well as from simple considerations, that all the water and all the conducting matter around the fish through which a discharge circuit can in any way be completed, is filled at the moment with circulating electric power; and this state might be easily represented generally in a diagram by drawing the lines of inductive action (1231. 1304. 1338.) upon it: in the case of a Gymnotus, surrounded equally in all directions by water, these would resemble generally, in disposition, the magnetic curves of a magnet, having the same straight or curved shape as the animal, i. e. provided he, in such cases, employed, as may be expected, his four electric organs at once.

1785. This Gymnotus can stun and kill fish which are in very various positions to its own body; but on one day when I saw it eat, its action seemed to me to be peculiar. A live fish about five inches in length, caught not half a minute before, was dropped into the tub. The Gymnotus instantly turned round in such a manner as to form a coil inclosing the fish, the latter representing a diameter across it; a shock passed, and there in an instant was the fish struck motionless, as if by lightning, in the midst of the waters, its side floating to the light. The Gymnotus made a turn or two to look for its prey, which having found he bolted, and then went searching about for more. A second smaller fish was given him, which being hurt in the conveyance, showed but little signs of life, and this he swallowed at once, apparently without shocking it. The coiling of the Gymnotus round its prey had, in this case, every appearance of being intentional on its part, to increase the force of the shock, and the action is evidently exceedingly well suited for that purpose (1783.), being in full accordance with the well-known laws of the discharge of currents in masses of conducting matter; and though the fish may not always put this artifice in practice, it is very probable he is aware of its advantage, and may resort to it in cases of need.

1786. Living as this animal does in the midst of such a good conductor as water, the first thoughts are thoughts of surprise that it can sensibly electrify anything, but a little consideration soon makes one conscious of many points of great beauty, illustrating the wisdom of the whole arrangement. Thus the very conducting power which the water has; that which it gives to the moistened skin of the fish or animal to be struck; the extent of surface by which the fish and the water conducting the charge to it are in contact; all conduce to favour and increase the shock upon the doomed animal, and are in the most perfect contrast with the inefficient state of things which would exist if the Gymnotus and the fish were surrounded by air; and at the same time that the power is one of low intensity, so that a dry skin wards it off, though a moist one conducts it (1760.): so is it one of great quantity (1770.), that though the surrounding water does conduct away much, enough to produce a full effect may take its course through the body of the fish that is to be caught for food, or the enemy that is to be conquered.

1787. Another remarkable result of the relation of the Gymnotus and its prey to the medium around them is, that the larger the fish to be killed or stunned, the greater will be the shock to which it is subject, though the Gymnotus may exert only an equal power; for the large fish has passing through its body those currents of electricity, which, in the case of a smaller one, would have been conveyed harmless by the water at its sides.

1788. The Gymnotus appears to be sensible when he has shocked an animal, being made conscious of it, probably, by the *mechanical impulse* he receives, caused by the spasms into which it is thrown. When I touched him with my hands, he gave me shock after shock; but when I touched him with glass rods, or the insulated conductors, he gave one or two shocks, felt by others having their hands in at a distance, but then ceased to exert the influence, as if made aware it had not the desired effect. Again, when he has been touched with the conductors several times, for experiments on the galvanometer or other apparatus, and appears to be languid or indifferent, and not willing to give shocks, yet being touched by the hands, they, by convulsive motion, have informed him that a sensitive thing was present, and he has quickly shown his power and his willingness to astonish the experimenter.

1789. It has been remarked by Geoffroy St. Hilaire, that the electric organs of the Torpedo, Gymnotus, and similar fishes, cannot be considered as essentially connected with those which are of high and direct importance to the life of the animal, but to belong rather to the common teguments; and it has also been found that such Torpedos as have been deprived of the use of their peculiar organs, have continued the functions of life quite as well as those in which they were allowed to remain.

These, with other considerations, lead me to look at these parts with a hope that they may upon close investigation prove to be a species of natural apparatus, by means of which we may apply the principles of *action and re-action* in the investigation of the nature of the *nervous influence*.

1790. The anatomical relation of the nervous system to the electric organ; the evident exhaustion of the nervous energy during the production of electricity in that organ; the apparently equivalent production of electricity in proportion to the quantity of nervous force consumed; the constant direction of the current produced, with its relation to what we may believe to be an equally constant direction of the nervous energy thrown into action at the same time; all induce me to believe, that it is not impossible but that, on passing electricity per force through the organ, a re-action back upon the nervous system belonging to it might take place, and that a restoration, to a greater or smaller degree, of that which the animal expends in the act of exciting a current, might perhaps be effected. We have the analogy in relation to heat and magnetism. **Seebeck** taught us how to commute heat into electricity; and **Peltier** has more lately given us the strict converse of this, and shown us how to convert the electricity into heat, including both its relation of hot and cold. **Oersted** showed how we were to convert electric into magnetic forces, and I had the delight of adding the other member of the full relation, by reacting back again and converting magnetic into electric forces. So perhaps in these organs, where nature has provided the apparatus by means of which the animal can exert and convert nervous into electric force, we may be able, possessing in that point of view a power far beyond that of the fish itself, to re-convert the electric into the nervous force.

1791. This may seem to some a very wild notion, as assuming that the nervous power is in some degree analogous to such powers as heat, electricity, and magnetism. I am only assuming it, however, as a reason for making certain experiments, which, according as they give positive or negative results, will regulate further expectation. And with respect to the nature of nervous power, that exertion of it which is conveyed along the nerves to the various organs which they excite into action, is not the direct principle of *life*; and therefore I see no natural reason why we should not be allowed in certain cases to *determine* as well as observe its course. Many philosophers think the power is electricity. **Priestley** put forth this view in 1774 in a very striking and distinct form, both as regards ordinary animals and those which are electric, like the Torpedo[*]. Dr. **Wilson Philip** considers that the agent in certain nerves is electricity modified by vital action[†]. **Matteucci** thinks that the nervous fluid or energy, in the nerves belonging to the electric organ at least, is electricity*. MM. Prevost and Dumas are of opinion that electricity moves in the nerves belonging to the muscles; and M. Prevost adduces a beautiful experiment, in which steel was magnetized, in proof of this view; which, if it should be confirmed by further observation and by other philosophers, is of the utmost consequence to the progress of this high branch of knowledge†. Now though I am not as yet convinced by the facts that the nervous fluid is only electricity, still I think that the agent in the nervous system may be an inorganic force; and if there be reasons for supposing that magnetism is a higher relation of force than electricity (1664. 1731. 1734.), so it may well be imagined, that the nervous power may be of a still more exalted character, and yet within the reach of experiment.

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[*] **Priestley** on Air, vol. i. p. 277. Edition of 1774.

[†] Dr. **Wilson Philip** is of opinion, that the nerves which excite the muscles and effect the chemical changes of the vital functions, operate by the electric power supplied by the brain and spinal marrow, in its effects, modified by the vital powers of the living animal; because he found, as he informs me, as early as 1815, that while the vital powers remain, all these functions can be as well performed by voltaic electricity after the removal of the nervous influence, as by that influence itself; and in the end of that year he presented a paper to the Royal Society, which was read at one of their meetings, giving an account of the experiments on which this position was founded.

1792. The kind of experiment I am bold enough to suggest is as follows. If a Gymnotus or Torpedo has been fatigued by frequent exertion of the electric organs, would the sending of currents of similar force to those he emits, or of other degrees of force, either continuously or intermittingly in the same direction as those he sends forth, restore him his powers and strength more rapidly than if he were left to his natural repose?

1793. Would sending currents through in the contrary direction exhaust the animal rapidly? There is, I think, reason to believe that the Torpedo (and perhaps the Gymnotus) is not much disturbed or excited by electric currents sent only through the electric organ; so that these experiments do not appear very difficult to make.

1794. The disposition of the organs in the Torpedo suggest still further experiments on the same principle. Thus when a current is sent in the natural direction, i. e. from below upwards through the organ on one side of the fish, will it excite the organ on the other side into action? or if sent through in the contrary direction, will it produce the same or any effect on that organ? Will it do so if the nerves proceeding to the organ or organs be tied? and will it do so after the animal has been so far exhausted by previous shocks as to be unable to throw the organ into action in any, or in a similar, degree of his own will?

1795. Such are some of the experiments which the conformation and relation of the electric organs of these fishes suggest, as being rational in their performance, and promising in anticipation. Others may not think of them as I do; but I can only say for myself, that were the means in my power, they are the very first that I would make.

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* Bibliothèque Universelle, 1837, tom. xii. 192.

† Ibid, 1837, xii. 202; xiv. 200.

Royal Institution,
November 9th, 1838.

II. *On the Application of the Conversion of Chlorates and Nitrates into Chlorides, and of Chlorides into Nitrates, to the determination of several equivalent numbers.*

By **Frederick Penny**, *Esq. Communicated by* **H. Hennell**, *Esq. F.R.S.*

Received January 17,—Read January 24, 1839.

1. The following researches originated from some experiments which were undertaken to discover an improved method for ascertaining the quantity of nitrate of potassa existing in crude saltpetre. After several unsuccessful attempts the action of hydrochloric acid was tried. The fact, that nitrates are decomposed by this acid, has been long known; but the nature of the resulting compound of potassium has not, so far as I am aware, been hitherto determined. I anticipated that the nitrate would be decomposed into chloride of potassium. To decide the question some pure nitrate of potassa was mixed with hydrochloric acid, and the mixture heated; while at common temperatures no perceptible action occurs, but immediately the acid becomes hot, decomposition commences. Chlorine and nitrous acid are evolved with copious effervescence, and the nitrate slowly disappears. The solution was gradually evaporated to dryness, and the dry salt treated with an additional quantity of acid until decomposition was no longer evident. The resulting salt was then carefully examined, and it was found to be pure chloride of potassium. This experiment was repeated several times, and all the results concurred in satisfactorily establishing the fact, that nitrate of potassa may be perfectly converted into chloride of potassium, provided a sufficient quantity of the acid be employed, and the temperature necessary to effect the decomposition be properly regulated.

2. So far the decomposition was admirably adapted for the object mentioned at the commencement. The usual impurities, such as chlorides, sulphates, silica, &c. which any sample of crude saltpetre might contain, would obviously remain unchanged, while the nitrate of potassa alone suffering decomposition, its quantity could easily be ascertained, by comparing the weight of the resulting salt with that obtained from a known quantity of absolutely pure nitrate. Several experiments were therefore performed to determine the exact quantity of chloride of potassium corresponding to a known weight of nitrate. The mean result of four experiments gave the ratio of 100 of nitrate to 73·730 chloride. I was then naturally led to compare this result with the equivalent numbers of these two compounds. In this country there are two series of equivalents in general use, one in which whole numbers are adopted, and the other in which fractional parts are admitted. For example, according to the former, nitrate of potassa will be 102, and to the latter 101·3. So chlo-

ride of potassium will be 76 and 74·6. Whence, according to the former, every 100 parts of nitrate should yield 74·51 of chloride: the latter gives the ratio of 100 to 73·613. But these results differ considerably from my experiments. In the one case, we have a difference of ·78, and in the other ·12. Whence therefore could they arise? Either the process must be defective, or the equivalent numbers, so generally considered as correct, must be erroneous.

3. To determine whether the process was inaccurate, I tried if chloride of potassium could be converted into nitrate of potassa by nitric acid. A single experiment upon some pure chloride decided in the affirmative; and the mean result of three experiments gave the ratio of 100 of nitrate to 73·727 of chloride. The correctness, therefore, of the experiments upon the nitrate was satisfactorily confirmed. Moreover in another experiment I converted a known weight of nitrate into chloride by hydrochloric acid, and then reconverted the resulting chloride into nitrate by nitric acid. The quantity of nitrate obtained was very nearly the same as that originally employed; there was not a hundredth of a grain difference. The error was thus traced to the equivalent numbers, but whether it existed in one, or more, of the elementary bodies constituting the above compounds, remained still unknown. The subject cannot be decided by the mere conversion of a nitrate into a chloride, or a chloride into a nitrate. Additional data are required. In order therefore to examine the question more scrupulously, the following experiments were undertaken; and as the results have proved consistent and satisfactory, I have presumed to submit them to the Royal Society.

4. In the present communication I propose to examine the equivalent numbers of oxygen, chlorine, nitrogen, potassium, sodium, and silver; and the following are the successive steps by which this examination has been conducted.

5. In the first place the equivalent of chloride of potassium will be determined, by decomposing the chlorate of potassa into oxygen, and chloride of potassium. From the ratio which these results bear to each other, the number for chloride of potassium, as compared with oxygen, may be easily deduced.

The same results will also give the number for chlorate of potassa.

Secondly. The equivalent of nitrate of potassa is ascertained. This is effected by converting the chlorate of potassa and the chloride of potassium into nitrate: and as we have already established the equivalents of chlorate and chloride, we can easily calculate the number for nitrate from these two methods of analysis. Moreover, from the equivalents of chlorate and nitrate of potassa, we can also learn the difference between the equivalents of chlorine and nitrogen, as the two salts agree exactly in composition, and only differ by nitrogen in the one being substituted for chlorine in the other. To confirm the experiments upon the conversion of chloride of potassium into nitrate, I have detailed some upon the decomposition of nitrate into chloride.

The process, hereafter described, by which these experiments were performed, was so exceedingly simple, that the preceding differences could not be referable to errors of manipulation.

Thirdly. The equivalents of nitrate, chlorate, and chloride of sodium are resolved. The method of investigation is similar to that adopted for the analogous salts of potassium. The chlorate of soda has been decomposed into chloride, and into nitrate; the nitrate of soda into chloride; and the chloride of sodium into nitrate of soda. The results of all these experiments are perfectly consistent with each other, and they confirm in the most satisfactory manner the accuracy of the experiments upon the salts of potassium.

Fourthly. The equivalent numbers of chlorine, nitrogen, potassium, and sodium have been ascertained. For this purpose the metal silver has been selected. The salts of potassium or sodium do not admit of application in this respect, as it is impracticable to ascertain with sufficient accuracy for the present purpose, the proportions in which either of these metals combine with chlorine or nitric acid. Bismuth was first selected from the facility of converting it into oxide, but the difficulty of obtaining this metal in a state of absolute purity, and its liability to form subsalts, induced me to prefer silver. I have accordingly determined the exact proportions in which this metal combines with chlorine and nitric acid; and lastly, to prove the correctness of the relation which the resulting nitrate bears to the chloride, I have performed several experiments upon the conversion of the crystallized nitrate into chloride. Perfect consistency exists throughout; and from these results, conjoined with those from the salts of potassium and sodium, the equivalents of chlorine, nitrogen, potassium, sodium, and silver are easily deduced.

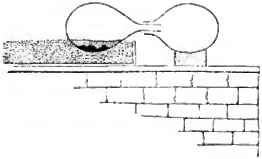
6. Such then is the arrangement of the evidence by which I propose to determine the equivalent numbers of these six elementary bodies; and to show that a slight alteration is necessary in those which are generally employed. Considering the number of distinguished chemists who have devoted their time and ability to the subject, it might naturally be expected that the truth had been satisfactorily established; but the slightest observation of the discrepancies in the results obtained, will prove the urgency of more refined investigations. In offering my feeble evidence, I am truly sensible of the responsible task I have undertaken. The well-attested skill of those from whom I shall have occasion to differ has constantly urged me to seek every means calculated to attain truth; and I trust, from the confirmatory nature of the evidence advanced, the numerous experiments performed, and the simplicity of the processes employed, that the results will be entitled to confidence.

7. In presenting this communication, I beg to acknowledge, with sincere gratitude, the constant assistance I have received from Mr. HENNELL; he has kindly watched the whole progress of the investigations, and by his advice has enabled me to overcome several difficulties. Let it not, however, be inferred that he is responsible for any of the faults which this communication may contain: they must rest entirely on myself.

8. The violent effervescence which occurs during the action of nitric acid upon a

chloride, or of hydrochloric acid upon a nitrate, except in the case of silver, appeared, at first, to offer an insuperable difficulty to the application of these actions to so delicate a purpose as the determination of equivalent numbers. It was requisite that some simple practical method should be devised to obviate this source of error. The unavoidable loss occurring during effervescence has rendered many chemical processes, which promise the most favourable results in theory, inapplicable to important investigations. In the experiments hereafter to be described, any error from this cause has been effectually prevented. A loss by spirting cannot occur. The substance is operated upon in a common flask placed in a horizontal position, with another attached to it as a receiver; the resulting substance is dried; and all the weighings are accomplished without removal from the flask. The performance and completion of the operation in one vessel is an additional advantage, and one which renders the process especially applicable to minute chemical researches. All those operations, such as transference, washing, filtering, &c. which introduce so many errors, are avoided. Little comment is required to show the advantages which the above process possesses in comparison to those hitherto applied to the determination of equivalent numbers. Few improvements, I imagine, can more increase the precision of our analyses, than a reduction of the number of operations by which these analyses are accomplished, combined with the employment of vessels which will bear a high temperature, resist the action of acids, and admit of being used with the most delicate balances. All these conditions have been fulfilled by the process employed in these researches, and as it has been applied to all the experiments, I shall premise its details thus early, to avoid unnecessary repetition.

9. Into a Florence flask, previously washed, dried, and tared, with the precautions hereafter stated, the substance to be operated upon was introduced, and its weight carefully ascertained. Another flask, with the whole of its neck removed, was also tared; and, as this was intended to be adapted to the other as a receiver, it was termed the receiver flask. The requisite quantity of acid was then poured upon the weighed substance, and the flask immediately placed in a horizontal position. The receiver flask was also adapted in the same position. If the decomposition would commence and continue without the assistance of heat, the flasks were allowed to remain as thus placed; but if heat were necessary, they were transferred without breaking the connexion to a sand bath. The arrangement is represented in the accompanying figure, and from an inspection of this, it can readily be perceived that a



loss by spirting cannot occur. The drops of liquid, being ejected vertically by the effervescence, are thrown against the upper side of the flask. Should any drops be thrown laterally, a circumstance of rare occurrence, they will be secured by the receiver. When the decomposition is completed, and the action has entirely ceased, the receiver is detached, and the solution cautiously evaporated to dryness. The receiver in the meantime is dried and re-weighed. During the evaporation of the excess of acid, a portion of the vapour condenses upon the upper side of the flask, and, returning to the solution, carries with it any salt that may have been thrown to the top. Moreover, from this internal washing of the flask, as it may not inaptly be termed, continuing during the whole of the evaporation, the dry salt, instead of being scattered over a considerable portion of the flask, is reduced to a small compact mass. Indeed it is beautiful to observe the small compass which the dry nitrates occupy from this cause. The flask and its contents are lastly heated in the flame of a spirit lamp, and the dry salt either fused or not according to its nature. After a sufficient time has been allowed for the cooling of the flask, it is returned to the balance, and the weight of the resulting compound carefully ascertained.

10. From the preceding process it will be remarked, that flasks are the only vessels which have been employed. Many precautions must be observed to ensure their successful employment. Those that I have used are of the capacity of a pint, and the lightest were always selected. Their average weight was about 320 grains. Perfect cleanliness was obtained by boiling nitric acid, and well rinsing with common, and afterwards with distilled water. They were dried by heating on a sand bath, and then in the flame of a spirit lamp. I have found it advantageous to remove one half the necks; for by this, their weights are materially lessened, and their dryness is more securely ensured. While cooling they were placed inside a glass case, free from dust, and were allowed to remain for two hours, in order to restore the equilibrium of the internal and external air. In ascertaining the weights of the flasks, it is better to weigh them against another flask, previously selected and dried for the purpose. This latter flask may be termed the gauge flask. It weighed 323·420 grains, and weights were of course added, on either side, to adjust the difference between it and the flask to be tared. The gauge flask was frequently reheated to insure accuracy, and when not in use was preserved from dust and damp under a bell glass placed inside a glass case. Before the final observations of the weighings were noted, the flasks were allowed to remain in the balance for a quarter of an hour. The success of the operations depends especially upon a strict observance of these precautions. The following circumstance will serve as an ample proof of the care required. Having observed in one of the experiments a small particle of dust upon the flask, I removed it from the balance, and wiped it as quickly as possible once round with a silk pocket handkerchief. The flask was held during this operation by the very extremity of the neck. Upon returning it to the scale it had lost ·025 of a grain, and this was owing to a slight expansion of the internal air, produced by the heat of the hand. It re-

gained its original weight in less than five minutes. By repeating this operation several times the flask lost nearly two tenths. The scales employed were made by Mr. **Robinson**, and would turn decidedly with the thousandth part of a grain, when loaded with five hundred grains. In the following experiments they were never loaded with so much. The weights were new, and by careful examination beforehand, were ascertained to be consistent.

Chlorate of Potassa.

11. It has already been stated, that the object with this salt, is its decomposition into oxygen and chloride of potassium. The method of accomplishing this hitherto has been to submit the salt to a high temperature; by which the oxygen is expelled and chloride of potassium remains. There are, however, many difficulties which render this process objectionable, and therefore I was induced to inquire whether the same result could be obtained by the action of hydrochloric acid. The changes occurring during the decomposition are well known. When the chlorate is in excess, the euchlorine of Sir **Humphry Davy**, or, as since proved by **Soubeiran**, a mixture of chlorine and chlorous acid, is evolved. If, however, the acid be in excess, chlorine only will be given off. All the oxygen of the chloric acid, and of the potassa, unites with the hydrogen of the hydrochloric acid, and forms water. The potassium remains combined with chlorine. I satisfied myself by numerous experiments that chloride of potassium is alone produced; and being enabled, by the process already described (9.), to counteract any error from the effervescence, I have adopted this method by hydrochloric acid in preference to that by heat.

12. The chlorate of potassa employed in the succeeding experiments was obtained pure by three successive crystallizations of ordinary commercial chlorate. After each crystallization the crystals were dried, rubbed to powder, and washed with distilled water. The salt which resulted from the third crystallization, dissolved without residue in water, and the solution was not affected by nitrate of silver or nitrate of baryta. Two hundred grains were converted by heat into chloride of potassium; the fused chloride was perfectly colourless, and when dissolved and tested not a trace of foreign matter could be detected.

13. The first experiments with this salt were rather discrepant, arising from my ignorance of the extreme caution which is requisite in drying it. Having observed in some chemical works that it is fusible at a temperature between 400° and 500° **Fahr.**, and will bear a temperature of 660° without undergoing decomposition, I conceived that very careful fusion would be the most secure method for ensuring dryness. The want of uniformity in the results, however, indicated some source of error, and upon examination I found that a very notable quantity of chloride of potassium was produced by the process of fusion, although effected with the utmost care. A temperature just below that of fusion was next tried, but upon dissolving the dry salt in distilled water, and testing with nitrate of silver, a minute portion of chloride could

be detected. Indeed it was not until after several trials that I succeeded in ascertaining the temperature at which this salt might be perfectly dried without risk of decomposition. This temperature is about 220°. The temperature of boiling water is quite safe: 1000 grains were kept in a copper pan, heated by steam, for fourteen hours without experiencing the slightest change. I have found by several experiments that one hundred grains of perfectly dried chlorate will lose about ·05 of a grain by fusion. The precaution of testing a portion of the dry salt was repeated after every drying, to ensure accuracy.

14. When dry, the powder was transferred, while hot, into a small tube, and tightly corked. As soon as cold, the quantity necessary for an experiment was transferred into a previously tared flask, and the weight of it ascertained. The tube was sufficiently small to pass down the neck of the flask, and almost to the bottom, so that any adhesion of the powder to the sides of the flask was prevented. The remainder of the process corresponded to the one already described, except that before adding the hydrochloric acid, it is expedient to fuse the chlorate in the flask. By this the action is very much moderated, and the chlorate is slowly and quietly dissolved. No heat is requisite if the acid be sufficiently strong.

15. The hydrochloric acid was prepared according to the general directions in a green glass retort. It was twice redistilled, and had a specific gravity of 1·160. It was carefully examined for sulphuric, nitric, and sulphurous acids, but not a trace of them could be detected. Two fluid ounces when evaporated left only ·015 of a grain of solid matter*.

16. Owing to the compactness of the mass in which the resulting chloride is left in the flask, a minute portion of hydrochloric acid remains adherent to the salt when its quantity is large; and this cannot be driven off without so high a temperature as to risk the cracking of the flask. To expel this adventitious acid, I have always adopted the following process. The mass, the weight of which has been ascertained, is detached from the side of the flask by gentle agitation, and broken into pieces sufficiently small to be removed. These pieces are then rubbed to powder in a glass mortar, and the powder is transferred into a previously tared platina crucible. Its weight is ascertained, and then it is heated to dull redness for half an hour. Fusion was carefully avoided, as it cannot be effected without a minute loss by sublimation. When cold the loss is easily determined, and as the weight of the salt in the crucible is known, it is easy to calculate the loss for the whole quantity originally in the flask. The quantity which remained adherent to the flask generally amounted to ·07 or ·08 of a grain.

17. I subjoin the results of six experiments, and annex, for the convenience of comparison, the calculations to 100 parts.

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* In the following experiments one fluid ounce of this acid was sufficient, so that not more than ·008 of a grain of impurity would be introduced, and this quantity was always subtracted from the resulting chloride.

Chlorate.	Chloride.	Chlorate.	Chloride.
76·626	46·598	or as 100 to	60·825
82·048	49·903	or as 100 to	60·822
75·200	45·733	or as 100 to	60·815
63·114	38·386	or as 100 to	60·820
61·164	37·202	or as 100 to	60·823
65·724	39·980	or as 100 to	60·830

The mean of these experiments indicates that 100 parts of chlorate of potassa contain 60·823 of chloride of potassium, and 39·177 of oxygen.

18. In another experiment I obtained the proportion of 100 of chlorate to 60·808 of chloride; but in this case the chloride was partially fused, and while so I detected the evolution of a small quantity of white vapour. It shows that 60·808 is too low. **Berzelius** obtained by heat 60·850 of chloride, and 39·150 of oxygen from 100 of chlorate.

19. It being well known that many powders, although not deliquescent, are more or less hygrometric, and no precautions having been taken to obviate this in the preceding experiments, I thought an error on this account might influence them. To decide the query, the following experiment was performed. Two hundred grains of the chlorate in powder were dried with every care, and transferred while hot into a stoppered phial, just large enough to contain it. When quite cold, the stopper was removed for a moment, and then the bottle and its contents, being placed in a small glass dish, the whole was counterpoised in the balance. The powder was transferred very carefully into the dish, without removal from the balance, and the empty bottle placed on top of the powder. The whole was left in this condition for two hours, and upon examination at the expiration of that time, not the slightest appreciable difference in weight could be detected. The experiment was twice repeated with the same result.

20. Perfectly dry powdered chloride of potassium was submitted to a similar experiment, but there was no evidence of change. If however the atmosphere be damp, both these salts when in powder rapidly absorb hygrometric moisture.

Conversion of Chlorate of Potassa into Nitrate.

21. Having determined, and I trust satisfactorily, that every 100 parts of chlorate correspond to 60·823 of chloride, the next object was to ascertain the quantity of nitrate resulting from 100 parts of the same salt. Its conversion into nitrate cannot however be performed directly, for the action of nitric acid upon a chlorate, as I shall show at another opportunity, is not to decompose it wholly into nitrate, but into a definite mixture of nitrate and perchlorate. The means by which the object now desired was accomplished, were by converting the chlorate into chloride, by hydrochloric acid, and then the resulting chloride into nitrate, by nitric acid. The operations were performed without any removal from the flask. The nitric acid was

added immediately the chloride was dry, without transferring and driving off the small quantity of adhering acid (16.). The minuteness of its quantity rendered it of no importance, as considerable excess of nitric acid was always employed. The nitrate produced was always fused to ensure dryness, and its weight being ascertained, it was dissolved and tested for chlorate and chloride. There was never the slightest indication of either. The impurities introduced by the acids, namely, one ounce of hydrochloric, and half an ounce of nitric, were of course allowed for. Only four experiments were performed, as their uniformity assured me of their close approximation to the truth.

22. The particulars of them are as follows:

Chlorate.	Nitrate.	Chlorate.	Nitrate.
86·454	71·329	100	82·505
70·205	57·918	100	82·497
62·244	51·368	100	82·498
72·636	59·938	100	82·500

The mean will be as 100 chlorate to 82·500 of nitrate.

23. Hereafter I shall show that 100 parts of nitrate correspond to 73·726 of chloride, so that the above result would indicate that 100 of chlorate contain 60·825 of chloride. This agrees very closely with the experiments by hydrochloric acid.

24. Moreover, this result, by comparison with that obtained in 17, shows, that

100 parts of chloride correspond to 135·640 of nitrate
and 100 parts of nitrate correspond to 73·724 chloride.

Chloride of Potassium.

25. Notwithstanding the experiments which have been made upon the action of nitric acid upon chlorides, much uncertainty still exists, as to the precise nature of the changes which occur. Their explication would dispel the doubt respecting the changes which take place during the mutual decomposition of nitric and hydrochloric acid, in the formation of nitro-muriatic acid. Sir H. Davy has stated that chlorine, nitrous acid, and water are produced. According to Dr. John Davy, a compound of chlorine and binoxide of nitrogen, with chlorine, and water result. Interesting as the decision of this question would be, I shall not now adduce the experiments I have made for this purpose, lest I should confuse the present inquiry. It has reference only to the ultimate result, which, as before stated (3.), is proved by the following experiment to be pure nitrate of potassa.

26. Two hundred grains of pure fused chloride of potassium were acted upon by one fluid ounce of nitric acid, sp. gr. 1·425; effervescence began immediately on mixture: abundance of chlorine tinged with nitrous acid was evolved. After the action had continued for two hours, a gentle heat was applied. There was a more copious evolution of nitrous acid, but the action soon ceased, and the solution remained co-

lourless. Evaporation was carried to dryness, and the dry salt fused. It was colourless, and weighed about 271·24. This corresponds closely to the quantity of nitrate which should be yielded according to the preceding experiments. Part of the fused salt was then dissolved in distilled water, and tested with nitrate of silver: not the slightest turbidity was evident. To another portion hydrochloric acid was added, to observe whether any chlorate of potassa had been formed, but not the slightest change was produced. The remaining portion upon being crystallized proved to be all nitre.

27. The chloride employed was prepared by saturating pure bicarbonate of potassa with excess of pure hydrochloric acid. The bicarbonate was purified by three crystallizations, and when tested no indication of foreign matter could be detected. The solution of chloride was evaporated to dryness, and the dry salt heated to expel the excess of acid. It was then dissolved in distilled water, and twice recrystallized. The salt, thus purified, was examined for sulphate, nitrate, lime, &c., but no impurity could be discovered. It was dried by careful fusion in the platina crucible. The process has already been detailed. Only half a fluid ounce of the nitric acid was employed in the following experiments. This nitric acid had been twice distilled and was perfectly colourless. It had a specific gravity 1·430. Two fluid ounces when evaporated left ·020 of a grain, and therefore not more than ·005 of a grain of impurity would be introduced.

28. The following are the particulars of the experiments performed:

Chloride.	Nitrate.	Chloride.	Nitrate.
84·090	113·058	100	135·639
61·998	84·092	100	135·637
51·823	70·293	100	135·640
67·145	91·072	100	135·635
55·350	75·071	100	135·630

whence, according to the mean,

100 of chloride correspond to 135·634 nitrate,
and 100 of nitrate correspond to 73·726 chloride.

29. In confirmation of these results, I procured some chloride from pure chlorate, by heating in the platina crucible, and fusing the resulting salt. I performed two experiments with this chloride and obtained the annexed results:

Chloride.	Nitrate.		
38·660	52·438	100	135·640
60·998	82·732	100	135·630

They correspond very closely to the preceding.

Nitrate of Potassa.

30. The conversion of this salt into chloride of potassium has been described, as well as the process by which it is effected. In addition to the precautions mentioned

in the process, there are several others to be observed to ensure success. As soon as the action commences, the flask should be removed from the sand bath, and the decomposition be permitted to continue without heat. When the action slackens, the flask may be returned to the sand bath, and so on, to and from the source of heat, until it is so gentle that no lateral spirting can be feared. The adaptation of a receiver throughout the whole of these operations must be strictly attended to; for despite every care, a minute portion of the solution will sometimes be ejected during the evaporation. This circumstance happened in three of the experiments I shall mention presently. In one case the weight was ·003 of a grain, in another ·008, and in a third ·005. The receiver flask should not be detached from the other even while removing them to and from the sand bath, nor in fact until the resulting chloride is nearly dry, as slight action continues during the greater part of the evaporation.

31. Very particular attention was paid to the purity of the salt employed. It was prepared from the common purified nitre of the shops, by four repeated crystallizations from distilled water. After each crystallization the salt was dried, and finely powdered, and this powder well washed with distilled water. When finally washed and dried, 200 grs. were fused in a platina crucible. They yielded a perfectly colourless mass, soluble in water without a trace of insoluble matter, and the solution was not affected by nitrate of silver, chloride of barium, oxalate of ammonia, or hydrosulphuret of ammonia. It was dried by careful fusion. The resulting chloride was dried in the same way as the chloride from chlorate. From one to two fluid ounces of hydrochloric acid are sufficient for the quantities of nitrate employed. In adding this acid one important practical circumstance must be observed, namely to add it in two successive portions, and to expel the first quantity previously to the addition of the second. This method is necessary from the small size of the flask, compared with the bulk of fluid. The less the quantity of fluid in the flask at one time, the less chance of loss by spirting.

32. I obtained, in seven experiments, the following very satisfactory results:

Nitrate.	Chloride.	Nitrate.	Chloride.
60·290	44·452	100	73·731
58·230	42·927	100	73·726
63·040	46·479	100	73·730
51·514	37·978	100	73·724
56·036	41·315	100	73·730
80·517	59·362	100	73·726
78·643	57·983	100	73·730

The mean will be as

100 of nitrate to 73·728 chloride,
or 100 of chloride to 135·634 nitrate.

33. Thus then we have ascertained, by three different methods, the ratio which chloride of potassium bears to nitrate of potassa. The results are as follows: from chlorate potassa (17.),

Nitrate. Chloride. Chloride.
100 corresponds to 73·724; or 100 to 135·640.

From chloride potassium (28.),

100 corresponds to 73·726; or 100 to 135·634.

From nitrate of potassa (32.),

100 corresponds to 73·728; or 100 to 135·633.

Whence the mean will be,

100 corresponds to 73·726; or 100 to 135·636.

34. Before quitting this salt, I cannot omit the opportunity of mentioning an instructive circumstance connected with my first experiments. The hydrochloric acid employed was obtained pure (as I then thought) by the redistillation of some ordinary pure acid. It was tested for nitric and sulphuric acids, but there was not a trace of either. Two fluid ounces yielded by evaporation only ·014 of impurities. I performed four operations, and the results were in the ratio of 100 of nitrate of potassa to

73·877 . . 73·888 . . . 73·886 . . . 73·877 of chloride.

The close approximation of these numbers inspired considerable confidence at the time, and in order to verify them, I converted a portion of pure chloride of potassium into nitrate. The result was as 100 of nitrate to 73·728 chloride. The difference between this and the above result indicated some source of error. The chloride, resulting from a nitrate experiment, was therefore reconverted into nitrate by nitric acid, but instead of obtaining the original quantity, I obtained much less. This circumstance suggested, that the error arose from some impurity in the materials, and suspecting the hydrochloric acid, a portion of it was submitted to another distillation. With this new acid another portion of nitre was decomposed, but the result was worse instead of better. The proportion was as 100 of nitrate to 74·04 of chloride. It was evident that some adventitious matter, more volatile than hydrochloric acid, was passing over with the acid in distillation. Sulphurous acid was suspected. A portion of the hydrochloric acid was boiled with a little nitric acid, diluted, and then tested with muriate of baryta; a precipitate of sulphate of baryta deposited. Therefore the acid did contain sulphurous acid, and this, by the action of nitric acid or a nitrate, was converted into sulphuric. The production of a higher result is therefore explained, and although vexatious at the time, it will now show the importance of obtaining the same result from different sources, and how admirably these reciprocal decompositions, of chlorides into nitrates and nitrates into chlorides, are adapted for this purpose.

Chlorate of Soda.

35. This salt has been very imperfectly examined. It is recommended to be prepared by passing chlorine through a solution of soda, and separating the chlorate and chloride by crystallization, or by saturating chloric acid with carbonate of soda. These processes, however, are very uncertain, and this may explain the contradictory descriptions given of this salt in chemical works. The process by which I succeeded in obtaining it pure, is as follows: some bitartrate of soda was prepared from carbonate of soda and tartaric acid, and purified by recrystallization. A quantity of pure chlorate of potassa was dissolved in a sufficient quantity of hot distilled water, and this solution while warm was mixed with a saturated hot solution of the above bitartrate of soda. Instant decomposition occurred; cream of tartar was precipitated, and chlorate of soda remained in solution. The whole was permitted to cool, and when quite cold the solution was decanted, evaporated, and crystallized. It is better to employ excess of chlorate of potassa: the proportions which I used were 32 of chlorate and 40 of bitartrate in crystals. The first crop of chlorate of soda generally contains a little bitartrate, but this may easily be separated by cold water, which dissolves the former and leaves the latter. The salt employed for the present inquiry was dissolved in spirit of wine, and afterwards twice recrystallized from distilled water. It was at last crystallized by spontaneous evaporation. The crystals thus obtained were very large. The diagonals of the largest face of many were not less than ¾ of an inch in length. They were either right square prisms or right rectangular prisms. I have never obtained them of any other shape. They contain no water of crystallization, and remain unchanged by exposure to the air: they fuse at a temperature a little higher than that required by chlorate of potassa; and, like this salt, they always undergo a slight decomposition by fusion.

36. Particular attention is required in concentrating the solutions of this salt; for if the heat be carried too far decomposition will take place. Oxygen in abundance is evolved, and chloride of sodium formed. When this has happened the chlorate is spoilt, for the almost equal solubility of the chlorate and chloride renders it impossible to effect their perfect separation. The criterion by which I judge of the proper strength of a solution, is blowing gently on the surface, and observing whether any crystals are formed; if so the heat may be removed.

37. The process for decomposition is the same as that employed for chlorate of potassa. In drying the resulting chloride of sodium, care must be taken to avoid a loss by decrepitation.

38. I subjoin the results of four experiments with the calculation to one hundred parts.

Chlorate.	Chloride.	Chlorate.	Chloride.
81·816	44·950	100	54·940
75·010	41·199	100	54·925
116·655	64·067	100	54·920
130·705	71·800	100	54·933

The mean will be very nearly 100 of chlorate to 54·930 of chloride.

39. The chlorate was next converted into nitrate by a process similar to that adopted for the analogous salt of potassa. I performed three experiments, and the following are the particulars.

Chlorate.	Nitrate.	Chlorate.	Nitrate.
110·843	88·536	100	79·875
118·290	94·492	100	79·882
89·112	65·198	100	79·890

The mean will be as 100 of chlorate to 79·882 chloride. And comparing this with the experiments on the conversion of chlorate into chloride,

100 parts of chloride will correspond to 145·425 nitrate,
and 100 parts of nitrate will correspond to 68·762 chloride.

Nitrate of Soda

40. Was prepared from pure carbonate of soda and nitric acid. The carbonate was obtained pure by repeated crystallizations of common carbonate. It was obtained in fine large crystals, and 200 grains when dried and fused yielded a colourless mass. This mass was then dissolved in distilled water and tested for chlorides, sulphates, lime, iron, &c., but no trace of impurity could be detected. The crystals were dissolved and saturated with pure nitric acid in excess; and the solution evaporated to dryness. The dry salt after heating to expel the excess of acid, was recrystallized three times. The resulting nitrate was quite pure, and was dried previously to use, by fusion in the platina crucible.

41. Its decomposition into chloride of sodium was effected by the same process as that applied to nitrate potassa. In drying the resulting chloride care must be taken to prevent a loss from decrepitation. Fusion must also be avoided, as the salt rapidly sublimes when fused. The hydrochloric acid was added in two portions, and from one ounce to one ounce and a half was sufficient for the quantities of nitrate employed.

42. I subjoin the results of six experiments with the calculations to 100 parts.

Nitrate of Soda.	Chloride Sodium.		
1. 56·606	38·926	100	68·767
2. 59·220	40·731	100	68·780
3. 50·316	34·602	100	68·770
4. 51·794	35·618	100	68·769
5. 55·250	37·995	100	68·770
6. 68·430	47·059	100	68·770

The mean of these results give the following proportions.

100 parts of nitrate soda correspond to 68·771 chloride sodium,
and 100 parts of chloride sodium correspond to 145·410 nitrate.

Chloride Sodium.

This salt was prepared by supersaturating pure carbonate of soda with pure hydrochloric acid. The solution was evaporated to dryness, and the dry salt, after heating to expel the excess of acid, was redissolved and recrystallized several times. The crystals obtained were quite pure, and were fused in the platina crucible to ensure dryness. It was converted into nitrate of soda by the same method as chloride of potassium. The resulting nitrate was fused in the flask. The following are the results of seven experiments. Number 3. was performed upon some chloride of sodium obtained from chlorate, by heating until all the oxygen was expelled. Number 7. was performed upon chloride in powder, dried in the platina crucible by the flame of a spirit lamp. Half an ounce of the nitric acid (27.) was sufficient for all the following experiments.

	Chloride.	Nitrate.	Chloride.	Nitrate.
1.	43·758	63·630	as 100 to	145·415
2.	50·454	73·465	as 100 to	145·408
3.	38·160	55·492	as 100 to	145·420
4.	48·914	71·112	as 100 to	145·424
5.	56·240	81·778	as 100 to	145·410
6.	58·942	74·021	as 100 to	145·418
7.	99·168	144·210	as 100 to	145·420

whence taking the mean

100 parts of chloride will correspond to 145·416 nitrate,
or 100 parts of nitrate will correspond to 68·768 chloride.

43. The conversion of chloride of sodium into nitrate of soda, affords one of the best and easiest methods for testing the accuracy of the equivalent numbers in general use. According to one series we have 60 as the number for chloride of sodium, and 86 for nitrate; another series gives 58·72, and 85·45 for the same compounds. Now taking these two series as the data of separate calculations, we find, that, according to the former, a hundred grains of chloride should yield 143·333 grains of nitrate; whereas the latter would give 145·504 grains of nitrate from the same quantity of chloride. Here then is a difference of nearly 2·2 grains;—a difference far exceeding any error of manipulation, and plainly indicating that one or other of these series must be incorrect. Upon which series the greater error is chargeable, every chemist may satisfy himself. The decomposition by nitric acid may be readily performed in flasks; and the only requisite for success is purity of materials.

Silver.

44. The object for which the following experiments were made upon this metal has been already stated (5.). The conversion of silver into chloride has been so frequently repeated by the most skilful chemists, that I conceived the better plan would

be to collate their results, and to take the mean as the basis of my calculation. But the discrepancies existing between them, and the objectionable processes which have in many cases been applied, urged me to examine the subject for myself.

45. The purity of the silver employed was very rigorously tested. One hundred and fifty grains were dissolved in pure dilute nitric acid; not a trace of insoluble black matter indicative of gold could be recognised. The nitric solution, being diluted, was precipitated by excess of pure hydrochloric acid; and the supernatant liquor, separated from the precipitate by filtration, was evaporated to dryness. A very minute quantity of solid matter remained; equivalent only to the impurities introduced by the acids and the water. It was examined for copper, lead, and iron, but not the slightest indication of their presence was afforded. The silver was in small pieces, and each piece was sufficient for an experiment.

Conversion into Nitrate.

46. The process for the conversion into nitrate was in all particulars identical to the one described in paragraph number 9. One fluid drachm of the pure nitric acid (27.) diluted with two drachms of distilled water was amply sufficient for the quantities of silver employed. With acid thus diluted the action is very gentle, and, if time be allowed, the whole of the silver may be dissolved without the assistance of heat. During the evaporation of the water and the excess of nitric acid, the receiver flask may be detached without any risk of loss. The dry nitrate was in all cases fused; but particular care is necessary to accomplish this operation successfully, for the salt being in a small cake, is, from the shape of the flask, much thinner at the edges than in the middle; and therefore, unless the heat be cautiously applied, a slight reduction into oxide will happen. This effect becomes evident by the fused nitrate being tinged with black. It only happened in one of the following experiments, and then a very minute black film could be detected on one side of the fused mass. During fusion and cooling, the flasks were always excluded from light.

47. The distilled water employed in these experiments had been twice redistilled in green glass, and four fluid ounces when evaporated left ·024 of a grain of solid matter. The quantity used, namely two drachms, would not introduce therefore a thousandth of a grain of impurity.

48. I subjoin the results of six from nine experiments, with the calculations to 100 parts.

	Silver.	Nitrate.	Silver.	Nitrate.
1.	59·087	93·020	or as 100 to	157·430
2.	60·311	94·952	or as 100 to	157·437
3.	51·654	81·333	or as 100 to	157·458
4.	55·734	86·765	or as 100 to	157·440
5.	45·622	71·827	or as 100 to	157·430
6.	64·726	101·913	or as 100 to	157·455

whence, taking the mean, 100 parts of silver correspond to 157·441 of nitrate.

Conversion into Chloride.

49. The quantity of chloride resulting from a known weight of silver was determined as follows. To each of the fused nitrates from several of the foregoing experiments (48.), half a fluid ounce of distilled water (47.) was added, and the salt dissolved with the assistance of a gentle heat. The solution being slightly acidulated with nitric acid, was precipitated by an excess of pure hydrochloric acid, amounting to half an ounce. These operations were performed without removal from the flask. The precipitated chloride, notwithstanding constant agitation during the addition of the hydrochloric acid, forms into a cake, and lest this should retain any undecomposed nitrate of silver, a glass rod of a dark green colour was introduced and the mass broken up. The rod, on removal from the flask, was always examined with a lens to see whether any chloride had adhered. The flask was next placed upon a heated sand bath, and the water and excess of acid cautiously expelled. The resulting chloride was beautifully white, and was always fused in the flask. During this operation, and while cooling, light was carefully excluded.

50. In the Philosophical Transactions for 1833, the late and much-lamented Dr. Turner has stated that chloride of silver may be dried perfectly and without the risk of decomposition at a temperature of 300° Fahr.; but he adds that even after this a slight loss occurs during fusion. With the latter part of these remarks my observations accord. In several experiments in which the chloride has been dried for a considerable time with a spirit lamp, guarding carefully against fusion, I have found that thus dried it always decreased slightly in weight by fusion, amounting to about ·006 of a grain for 70 grains. Dr. Turner attributed this loss to a slight decomposition. I have repeatedly tried to satisfy myself on this point, but have hitherto been unsuccessful. I have never seen any evidence of decomposition.

51. In five experiments performed as already stated, I obtained the following results.

	Silver.	Chloride.	Silver.	Chloride.
1.	59·087	78·489	or 100 to	132·836
2.	60·311	80·117	or 100 to	132·840
3.	51·654	68·612	or 100 to	132·830
4.	55·734	74·037	or 100 to	132·840
5.	64·726	85·982	or 100 to	132·840

52. Again, in two additional experiments I dissolved the silver in a sufficient quantity of dilute nitric acid, and precipitated at once with hydrochloric acid, without evaporating and procuring the nitrate. These are the particulars of the experiments.

Silver.	Chloride.		
57·882	76·884	100	132·830
51·380	68·252	100	132·838

Taking the mean of the seven experiments, 100 parts of silver will correspond to 132·836 of chloride.

53. The mean of three experiments, described by Dr. Turner in the Philosophical Transactions for 1829, gives the ratio of 100 of silver to 132·835 of chloride. The following table, extracted from Professor Brande's Manual of Chemistry, will show the results of different experimenters upon the same subject.

Dr. Thompson	100 of silver to 133·333 of chloride.
Rose	100 of silver to 133·014 of chloride.
Berzelius	100 of silver to 132·736 of chloride.
Gay Lussac	100 of silver to 132·890 of chloride.
Marcet and Davy	100 of silver to 132·450 of chloride.

54. From the five experiments on the chloride, we can also ascertain the ratio of nitrate of silver to chloride. For as the silver employed had been previously converted into nitrate, we can, by comparing the mean result from the nitrates with that from the chloride, obtain the ratio required. Thus every 100 parts of silver correspond to 132·836 of chloride and to 157·441 of nitrate; whence,

100 of chloride will correspond to 118·523 nitrate,
and 100 of nitrate will correspond to 84·372 chloride.

Nitrate Silver.

55. That 100 parts of nitrate to 84·372 of chloride is near the truth, the following experiments on the crystallized nitrate satisfactorily confirm. The nitrate of silver was obtained from pure commercial silver by solution in nitric acid and crystallyzing. After three crystallizations it was obtained in fine bold crystals, and was quite pure. Its perfect dryness was ensured by fusion in a small glass tube. To convert it into chloride, the same process and precautions were adopted as have already been described. The chloride was always fused.

56. I subjoin the results of five experiments.

Nitrate.	Chloride.	Nitrate.	Chloride.
93·452	78·847	100	84·370
115·414	97·395	100	84·388
65·500	55·267	100	84·377
93·034	78·490	100	84·367
108·645	91·664	100	84·370

The mean of these experiments will be

100 of chloride to 118·520 nitrate,
and 100 of nitrate to 84·374 chloride.

57. Dr. Turner, in the Philosophical Transactions for 1829, gives the details of two experiments upon the conversion of nitrate of silver into chloride. According to one, 100 parts of nitrate correspond to 84·357 of chloride; and to the other, 100 to 84·389. The mean will be 100 to 84·373. The close accordance of the above results

not only proves the correctness of the experiments, but also the purity of the materials.

58. It now remains to determine from the foregoing experimental data, the equivalent numbers of the several compounds described, and of the elementary bodies of which these compounds are constituted. For the convenience of reference, I have arranged the results in a tabular form, and have included the results corresponding to the two series of numbers in general use in this country. I have taken the liberty to name them according to their respective authors.

	Calculated according to Dr. Thompson.	Calculated according to Dr. Turner.	According to the foregoing experiments.
100 parts of chlorate of potassa yield of chloride	61·290	60·838	60·825
100 parts of chlorate of potassa yield of nitrate	82·259	82·646	82·500
100 parts of chloride of potassium yield of nitrate	134·210	135·845	135·636
100 parts of nitrate of potassa yield of chloride	74·510	73·613	73·726
100 parts of chlorate of soda yield of chloride	55·555	55·022	54·930
100 parts of chlorate of soda yield of nitrate	79·630	81·942	79·882
100 parts of chloride of sodium yield of nitrate	143·333	145·504	145·414
100 parts of nitrate of soda yield of chloride	69·767	68·666	68·771
100 parts of silver yield of nitrate	156·363	157·546	157·441
100 parts of silver yield of chloride	133·333	132·796	132·837
100 parts of nitrate of silver yield of chloride	85·272	84·290	84·374
100 parts of chloride of silver yield of nitrate	117·808	118·637	118·520

59. Of the six elementary substances included in the above compounds, there is only one upon the number of which chemists are agreed. That is oxygen. Upon a scale in which hydrogen is considered as unity, oxygen is stated to be eight; and as this number is generally employed in this country, we may adopt it as the basis of the calculations in the present inquiry. Should it be proved erroneous by future investigations, the numbers, presently deduced, must undergo a proportionate alteration. Remembering, therefore, that I have merely assumed the number eight for oxygen to be correct, we may proceed according to the method already detailed (5.) to ascertain the several equivalents.

60. Chloride of Potassium. According to the preceding table 100 parts of chlorate of potassa correspond to 60·825 of chloride, and therefore the same quantity of chlorate will contain 39·175 oxygen. But in every proportional of chlorate, there are admitted to be six proportionals of oxygen; whence as 39·175 : 60·825 : : 48 : to the equivalent of chloride of potassium. By performing the operation we obtain the number 74·527. Chlorate of potassa will therefore be 74·527 + 48 = 122·527. Nitrate of potassa is determined by the ratio which chloride bears to nitrate, thus according to the table 100 of chloride correspond to 135·636 of nitrate; and therefore as 100 : 135·636 : : 74·527 : 101·087, which will be the equivalent of nitre. Moreover from what has been stated (5.) we learn, that 122·527, the equivalent of chlorate, minus 101·087 the equivalent of nitrate, equals the difference between chlorine, and nitrogen, namely 21·440;—and also that 101·087, minus 74·527 the number for

chloride, is the difference between one proportional of nitrogen plus six of oxygen minus chlorine: namely 26·560.

61. By performing the same method of calculation with the results from the salts of sodium, we have as follows:

Chloride of sodium	58·500
Chlorate of soda	106·500
Nitrate of soda	85·068
Chlorine — nitrogen	21·432
Nitrogen + 6 oxygen — chlorine	26·568

The close accordance of these latter differences with those from the salts of potassium cannot fail, I think, to inspire confidence in the accuracy of the experiments.

62. From these differences we may readily determine the numbers for the salts of silver as well as those for chlorine and nitrogen. Thus, according to the table, 132·837 of chloride of silver correspond to 157·441 of nitrate: the difference being 24·604. But we have ascertained that the difference between a nitrate and a chloride is 26·565, and therefore as 24·604 : 157·441 or 132·837 : : 26·565 : to the equivalent of chloride or of nitrate of silver. Whence the nitrate equals 169·989 and the chloride 143·424; and as 132·837 of chloride contain 100 of silver, therefore the number for silver is 107·970.

63. Nitrogen will be 14·02 and chlorine 35·454. Potassium therefore is 39·073, and sodium 23·046.

64. For the convenience of reference I have subjoined the numbers of these elementary bodies in the following table; with the numbers given by the two distinguished chemists whose names I have had occasion to mention.

	Thompson.	Turner.	Penny.
Oxygen	8	8	8
Chlorine	36	35·42	35·45
Nitrogen	14	14·15	14·02
Potassium	40	39·15	39·08
Sodium	24	23·3	23·05
Silver	110	108·0	107·97

65. These researches corroborate the conclusions to which Dr. Turner was led by his experimental inquiries, published in the Philosophical Transactions for 1833. They show that the estimates in general use among British chemists are not the strict representatives of chemical truth, founded on experiment; and that the favourite hypothesis, of all equivalents being simple multiples of hydrogen, is no longer tenable. My estimates of chlorine and silver correspond very closely to those of Dr. Turner. His number for nitrogen was deduced from experiments on the nitrates of silver, lead, and baryta. He obtained the numbers 14·09, 14·17, 14·2, and he adopted 14·15 as the mean. I have carefully re-examined my experiments, but I cannot discover

anything to justify an alteration of the number 14·02. The experiments upon the conversion of silver into nitrate have satisfied me that 14·15 for nitrogen is inaccurate; for by reference to the table in page 31, it will be seen that according to this number I should have obtained 157·546 of nitrate from 100 of silver, whereas I obtained 157·441. The difference, namely, 0·1, is too high to be referable to errors of manipulation, considering the simplicity of the process employed. Dr. Turner's numbers for potassium and sodium are not founded upon any experiments of his own, but are extracted from Berzelius's Tables of Equivalents.

66. In conclusion, I may mention the several additional subjects which the present inquiry has elicited for investigation. I have extended the decomposition of nitrates into chlorides, and chlorides into nitrates, to the salts of baryta, lime, strontia, manganese, and lead; but whether these salts are applicable to the determination of equivalent numbers, must be decided by future experiments.

67. I have also satisfied myself, that the same method of investigation may be applied to several iodates and iodides, bromates and bromides. They are converted into chlorides by hydrochloric acid; and the iodides and bromides may be converted into nitrates by nitric acid. These decompositions afford excellent means of determining the equivalents of iodine and bromine, and of corroborating the experiments already described upon the nitrates and chlorides. For suppose that two equal quantities of pure iodate or bromate of potassa are converted into chloride and nitrate, it is obvious that the quantities of these resulting salts should bear the same ratio to each other as that established in the present paper. The same ratio should result from the iodides and bromides, if the experiments be correctly performed. Opportunity, however, will not permit me at present to complete the experiments undertaken upon these salts. The special care required in the preparation of the materials, the number of experiments to be performed, and the extreme delicacy necessary to attain truth, would occupy so much time, that I have deemed it better to communicate the results already obtained, than to wait for the completion of the whole. Moreover the details would far exceed the just limits of a single communication.

68. I have likewise examined the action of nitric acid upon chlorates and iodates, and have obtained some novel and interesting results. The process so frequently referred to has also enabled me to make some satisfactory experiments upon the conversion of carbonate of soda into nitrate and chloride, and to obtain some important evidence respecting the equivalent of carbon.

69. As soon as opportunity will permit me to resume and complete these several investigations, I shall do myself the honour of communicating them to the Royal Society.

Apothecaries' Hall,
January 10th, 1839.

III. *Researches on the Chemical Equivalents of Certain Bodies.* *By* Richard Phillips, *F.R.S., &c.*

Received January 17,—Read February 14, 1839.

THE late Dr. Turner, in an elaborate memoir on atomic weights, contained in the Philosophical Transactions for 1833, observes, that "Dr. Prout's hypothesis as advocated by Dr. Thomson, that all atomic weights are simple multiples of that of hydrogen, can no longer be maintained," and he further asserts that hypothesis "to be at variance with the most exact analytic researches which have been conducted."

Although the experiments of Dr. Turner, and the inferences which he has drawn from them, agree very nearly with those of Berzelius, it still appeared desirable further to investigate this subject, and it occurred to me that the inquiry might be conducted in a mode not liable to some of the objections which may be urged against the processses usually adopted.

For the purposes of this investigation, I deem it peculiarly fortunate that Dr. Turner has adopted a whole number as the equivalent of silver, and on this account, as well as for other obvious reasons, I selected it as the basis for an inquiry respecting the equivalents of chlorine and some other elementary gases.

From Dr. Turner's experiments (Philosophical Transactions, 1829), it appears that 108 parts, or one equivalent of silver, yield 143·424 parts of fused chloride, which result coincides very nearly with the determination of Berzelius. Dr. Prout, however, has objected to the fusing of the chloride of silver, that during the operation, it yields hydrochloric acid, which is admitted by Dr. Turner to be the case, although he could not discover that chloride of silver, that had been dried at 300°, lost so much as a thousandth of its weight by subsequent fusion.

It seemed to me that the chance of error arising from the fusing of the chloride of silver might be entirely removed, and other advantages gained, by making an experiment with silver on a large scale, with such proportions of the substances employed as were deemed to be equivalents; and instead of calculating from the whole product of the fused chloride, to do it merely from the weight of such small portion only as might arise from the difference between theoretical views and experimental results.

With this purpose I purified some silver by dissolving it in nitric acid, precipitating by a chloride, dissolving the precipitate in ammonia, again throwing down the chloride and reducing it to the metallic state.

I could not discover any impurity in the silver thus obtained: I therefore dissolved 216 grains (2 equivalents) in nitric acid, and decomposed the nitrate formed, by adding 108 grains of pure hydrochlorate of ammonia dissolved in water. I need hardly state, that the weight of the hydrochlorate was taken on the assumption, that this salt contains one equivalent of chlorine 36, one of azote 14, and four equivalents of hydrogen 4, equal 54. The chloride of silver precipitated was collected and washed on a filter, but instead of drying and fusing it, it was neglected, for a reason already assigned. Having ascertained that the solution from which this chloride was precipitated contained excess of hydrochlorate of ammonia, I added nitrate of silver to it and to the washings; the chloride of silver thus obtained was collected on a double filter of Chinese paper and washed and dried; it weighed 2·58 grains, a quantity too small to admit of any appreciable error from deficient drying in not subjecting it to fusion. Assuming that 144 of chloride of silver indicate 36 of chlorine, 2·58 grains will give 0·645 grain as the weight of the excess of chlorine in the two presumed equivalents of hydrochlorate of ammonia, and one half of this, or 0·322 grain, subtracted from 36, will reduce the equivalent of chlorine from that number to 35·678. On repeating this experiment I obtained 2·56 grains of chloride of silver, which brings the equivalent of chlorine to 35·680.

In order to bring under discussion the equivalents of other elementary bodies, I prepared some nitrate of silver from the pure metal; it was twice crystallized, and a portion of it, fused in a glass capsule, weighed 271·57 grains; adopting the equivalent weights above stated, and 8 for oxygen, 54 of hydrochlorate of ammonia should decompose 170 of nitrate of silver, and 86·263 of the hydrochlorate would therefore be required for the decomposition of the fused nitrate.

The solutions of these quantities of the salts were accordingly mixed, the precipitate was, as before, separated and washed, and on adding nitrate of silver to the filtered solution and washings, 1·74 grain of chloride of silver was obtained, indicating an excess of 0·435 grain of chlorine; now according to the assumed equivalents, 54 of hydrochlorate of ammonia contains 36 of chlorine, 86·263, the quantity employed in the experiment, therefore contained 57·508, from which, if we subtract 0·435, the excess, there will remain 57·073; if then 86·263 give 57·073, 54, the equivalent of hydrochlorate of ammonia, will contain 35·727 of chlorine instead of 36. This experiment was repeated with 357·34 grains of fused nitrate of silver, and 113·508 grains of hydrochlorate of ammonia; the excess of chlorine was 0·7 grain, which calculating as before, makes the equivalent of chlorine 35·667; and taking the mean results of the four experiments, viz. 35·678, 35·680, 35·727, and 35·667, we have 35·688 as the equivalent of chlorine.

Although Dr. Prout's hypothesis requires that the error, or the difference between 35·688 and 36 = 0·312, should not be divided among the various substances employed in the experiment, it may, nevertheless, be worth while to observe to what the error amounts when the numbers and quantities of the elements included in the operation are summed up: they are

1 equiv. silver . . .	108	
1 equiv. chlorine . .	36,	in the hydrochlorate of ammonia.
4 equivs. hydrogen. .	4,	one in the acid, three in the alkali.
6 equivs. oxygen . .	48,	one with the silver, five in the acid.
2 equivs. azote . . .	28,	one in the acid, one in the alkali.
14 equivs. weighing . .	224	

We have thus 14 equivalents of various elements, the sum of whose weights is 224, yet the mean error, or 0·312, without allowing for circumstances which I shall presently notice, is only about 1–717th part of the whole weight. It may also be remarked, that omitting the silver, and considering the other elements in their gaseous state, the error in volume will be comparatively less than that in weight; though it must at the same time be admitted that this difference is derived from the greater density of chlorine than of the other gases.

1 equiv. chlorine	36 grains	=	46 cubic inches, nearly.
4 equivs. hydrogen . . .	4 grains	=	186 cubic inches.
6 equivs. oxygen	48 grains	=	139 cubic inches.
2 equivs. azote	28 grains	=	93 cubic inches.
	Total		464 cubic inches.

The weight of chlorine in error being, as already mentioned, 0·312 grain, or about 0·4 cubic inch, is only 1–1160th of the whole volume of the gases.

I have every reason to believe that all the substances which I used in the experiments above detailed, were of the greatest degree of purity; but I may observe that the admixtures most likely to occur in any of them would increase the quantity of the chlorine, or diminish that of the silver; and any substance producing either of these effects would erroneously diminish the equivalent of chlorine by increasing the weight of the precipitated chloride of silver, assumed to be derived from the error of the theory; thus hydrochlorate of ammonia always contains sufficient excess of acid to redden litmus paper, and any moisture or foreign metal which the nitrate of silver might contain would produce corresponding results.

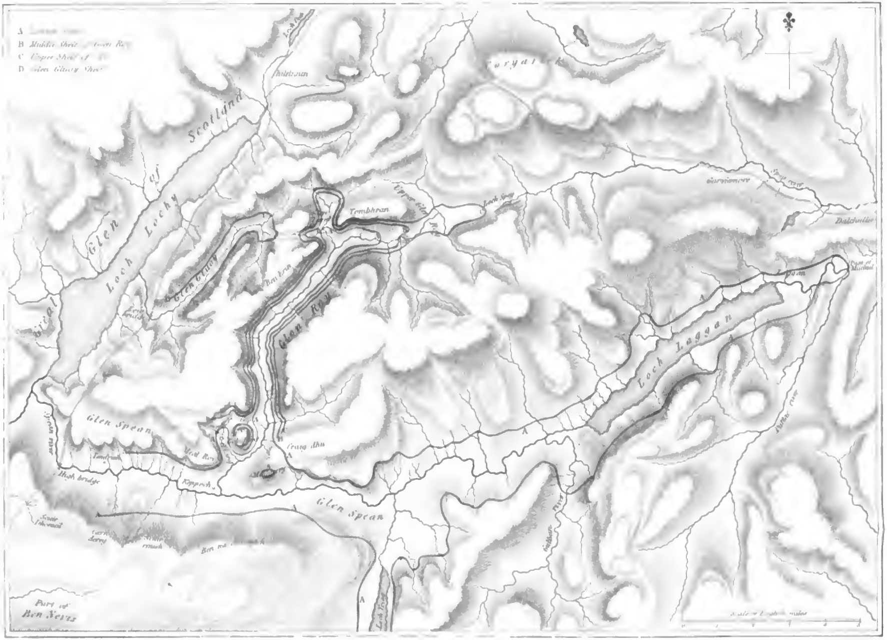
From these experiments and considerations I am of opinion, that no material, and scarcely even any appreciable error can arise from considering the equivalents of hydrogen, oxygen, azote, and chlorine, as 1, 8, 14, and 36 respectively.

The specific gravity of oxygen and azote may be obtained by comparing their equivalent weights with the composition and density of atmospheric air. The mean of various experimental results of the weight of 100 cubic inches of air is about 31 grains, and this agrees very nearly with the determination of Dr. Prout. Now if pure air consist of 20 cubic inches of oxygen and 80 of azote, or of one equivalent of oxygen and 2 equivalents of azote, the weights of which are respectively 8 and 14, the 20 cubic inches of oxygen will weigh 6·88 grains, and the 80 of azote 24·08 grains, giving 30·96 grains as the weight of 100 cubic inches of air; if, as more commonly admitted, air consists of 21 volumes of oxygen and 79 of azote, we shall have 7·224 grains as the weight of the oxygen, and 23·779 as that of the azote = 31·003 grains as the weight of 100 cubic inches of air; either of these determinations is sufficiently near to show, that we cannot be far wrong in estimating the weight of 100 cubic inches of oxygen at 34·4 grains, and the same volume of azote at 30·1 grains; and the weight of the equivalent of hydrogen being $\frac{1}{8}$ that of oxygen, and its volume twice as great, and the equivalent of chlorine being to that of oxygen as 36 to 8, while their volumes are equal, it follows that 100 cubic inches of hydrogen and chlorine will weigh respectively 2·15 and 77·4 grains, and the density of the four elementary gases in question, compared with air = 1, will be

Hydrogen	0·06935
Azote	0·97097
Oxygen	1·10968
Chlorine	2·49678

On comparing these densities with those stated by Professors **Thomson**, **Turner**, and **Graham**, it will be observed that they are less than those given by Dr. **Thomson**, which is accounted for by his having assumed that 100 cubic inches of air weigh 31·1446 grains, while they vary, but not very materially, from the weights assumed by Professors **Turner** and **Graham**, in being in some cases rather lighter and in others somewhat heavier.

	Thomson.	Turner.	Graham.
Hydrogen	0·0694	0·0690	0·069
Azote	0·9722	0·9727	0·976
Oxygen	1·1111	1·1025	1·1026
Chlorine	2·5000	2·4700	2·470



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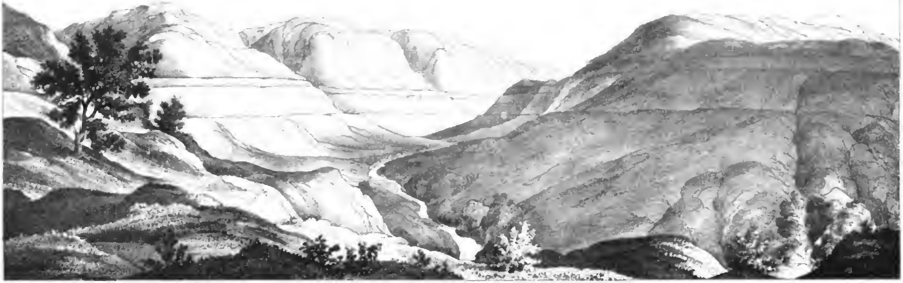
Parallel Roads or Shelves of Glen Roy and the adjoining Valleys

Phil. Trans. MDCCCXXXIX. Plate I. p. 39



Albert Way, delᵗ

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View of the Parallel Roads of Glen Roy

IV. *Observations on the Parallel Roads of Glen Roy, and of other parts of Lochaber in Scotland, with an attempt to prove that they are of marine origin. By* Charles Darwin, *Esq., M.A. F.R.S. Sec. G.S.*

Received January 17th,—Read February 7th, 1839.

After the two elaborate memoirs which were read nearly at the same time, before the Edinburgh Royal Society and the Geological Society of London, by Sir Thomas Lauder Dick and Dr. Macculloch, on the parallel roads of Glen Roy and the neighbouring valleys, any detailed account of the physical structure of that remarkable district would be superfluous. But from the excellence of these papers and the high authority of their authors, it is necessary carefully to consider the theories they have advanced,—a necessity I feel the more strongly, from having been convinced during the few first days of my examination of the district, that their conclusions were impregnable. Moreover the results to which I have arrived, if proved, are of so much greater geological importance than the mere explaining the origin of the *roads*, that I must beg to be permitted to enter into the subject in detail.

Section I.—*Description of the Shelves.*

The parallel roads, shelves, or lines, as they have been indifferently called, are most plainly developed in Glen Roy. They extend in lines, absolutely horizontal, along the steep grassy sides of the mountains, which are covered with a mantle, unusually thick, of slightly argillaceous alluvium. They consist of narrow terraces, which, however, are never quite flat like artificial ones, but gently slope towards the valley, with an average breadth of about sixty feet. There are only four shelves which are *plainly* marked for any considerable length; the lowest one according to Macculloch is 972 feet above the sea; the next above it is 212 feet higher, and the third, eighty-two above the second, or 1266* above the sea; the fourth occurs only in Glen Gluoy; it is twelve feet higher than the third. I shall refer to them either by their absolute altitude, or as being the upper or lower one in the part under description, and not as first, second, or third; for it will be hereafter seen that others occur in every respect similar, only less plainly developed.

It is admitted by every one, that no other cause, except water acting for some period on the steep side of the mountains, could have traced these lines over an extensive district. The dark line in the accompanying wood-cut (No. 1.) represents the real profile of a shelf, and is copied from Macculloch. To this I have added two imaginary lines, of which the broken one gives the supposed original form of the underlying rock. The formation of the shelf, as may be here seen, is chiefly due to the accumulation of matter in the form of a mound, only very slightly projecting beyond the general slope of the mountain, and partly to the removal or corrosion of the solid rock. The latter effect, although well marked in some particular spots, cannot generally be distinguished, and the shelves no doubt are chiefly due to the accumulation, and not to the removal of matter. In this same diagram (1.) the covering of alluvium is represented as thicker some way below the shelf, than at the same distance above it. I believe this is generally the case, and hence the projection of the shelf is often very obscure; and when two or three occur, one below the other, their outline closely approaches to that represented in wood-cut (2.). Macculloch will scarcely even allow that a shelf in any case forms a projecting mound; but this certainly is incorrect, and is indeed contradicted by his own statements, and by that implied in the comparison of the shelves with the beaches of lakes, which have been suddenly drained. The shelves entirely disappear, where crossing any part of the mountains in which the bare rock is exposed; for loose matter cannot accumulate there, and the rocks themselves from their laminated structure do not readily become worn into any regular form. They likewise disappear where crossing any part which is gently inclined; for their own slope then coincides with that of the alluvial covering, and cannot be distinguished from it.

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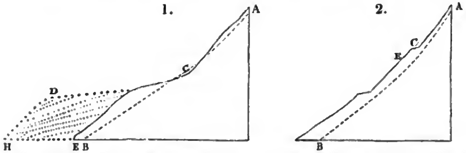
* Some rude measurements which I made with a mountain barometer lead me to suspect that these altitudes are at least a hundred feet too great. It is not a point of any importance with respect to the theory of the origin of the shelves, but I regret that I did not verify their height with more care.



A B supposed original surface of rock; C E line of shelf; C D H line of shelf when expanded into a buttress or terrace.

The *dotted* line in the wood-cut (No. 1.) is supposed to represent the broader terraces, or even plains, of stratified shingle, sand, and mud, with which the shelves often become united. These terraces do not differ from the shelves in any one essential point of structure, but are much broader; and as the matter of which they are composed is in much larger quantity, a rude kind of stratification may be generally observed. They occur only where the bottom of the valley in its gradual ascent rises nearly to the level of a shelf, or at points on the hill-sides, where it is probable that the streamlets formerly brought down much detritus to the ancient beaches.

Sir **Lauder Dick** has observed* that the shelf infallibly intersects the head of such terraces or buttresses: this certainly is the case (as in diagram 1.) with all the smaller ones; and, therefore, we may infer that their formation dates from the period when the shelf was a beach. But at the head of the greater valleys, where the supply of matter must have been more abundant, and where the slope of the land was highly favourable to its accumulation, the line of shelf sweeps across and blends into a plain, which has an uniform slope upwards and downwards above and below that level. Therefore, when the water stood at any one of the shelves, there were many little deltas which did not rise above its level, but some greater ones that were continuous with an upward slope of shingle, filling the bottoms of the main valleys†.

The shelves are chiefly composed of the same kind of alluvium with that covering the whole surface of the mountain; and they seem to have been formed, as suggested by **Macculloch**, by the check given to the downward descent of ordinary detritus, and that transported by torrents, at the level of the ancient waters; I could perceive no difference in the nature of the alluvium above and below the upper shelf, as stated to be the case by Sir **Lauder**‡. It contains fewer well-rounded pebbles at the greater heights than would have been expected on any theory of the origin of the shelves; but they are abundant in the lower and broader parts of the valleys. Nevertheless where there is any level spot at the height of the upper shelves, well-rounded pebbles may generally be found, as on the summit of a rounded hill, or a flat little strait separating some hillock from a line of shelf (for instance near Craigdhu, on the summit of Meal Roy, and between Upper and Lower Glen Roy). In these cases the pebbles must have been almost exclusively formed by the action of the currents and waves of the former expanse of water. They are frequently derived from rocks not found in the immediate vicinity: erratic boulders also are scattered over these mountains. I state these facts distinctly, because **Macculloch** says§ that the composition of the alluvium of the upper shelves is *wholly* different from that covering the sides of the broad valleys; whereas the difference is only one of degree, for which many causes might be assigned.

I have already observed, that the quantity of solid rock worn away on the line of any shelf is not usually great. At the narrow entrance, however, of Loch Treig (of which a drawing is given by Sir **Lauder Dick**), on the west side, which is very steep, the gneiss is worn into smooth concave hollows, the peculiar curves of which, though they cannot be described, may be readily imagined by calling to mind the form of rocks washed by a water-fall. This was the only one spot where I could observe this appearance in an unequivocal manner; but this one point of rock would to my mind carry demonstration with it, even if there were not innumerable other proofs, that the water had remained at the level of the 972 feet shelf for a very long period*. On the opposite side of the entrance, or gorge, which here slightly bends before entering Loch Treig, the shelf expands into a line of terrace. Standing on the precipitous and waterworn rocks, it required little imagination to go back to former ages, and to behold the water eddying and splashing against the steep rocks on one side of the channel, whilst on the other it was flowing quietly over a shelving spit of sand and gravel. The only other and rather different case of waterworn rock, which I noticed, was at the head of Lower Glen Roy (pointed out by Sir LAUDER DICK), where the summits of some irregular hummocks of gneiss on a level with the upper shelf were obliquely truncated by a smooth surface. I have frequently observed a similar structure on the rocky shores of protected harbours. Large fragments of rock are scattered on most of the shelves, of which many are of granite, and have come from a distance, as will presently be described; the greater number, however, have merely rolled down from the heights above. Of the latter, some have fallen recently, whilst others are waterworn, as if they had lain for centuries on a sea coast; and it was in many cases easy to point out, whilst walking along the level shelf, which fragments had been washed by the ancient waves, and which had fallen since.

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* Transactions of the Edinburgh Royal Society, vol. ix. p. 11.

† These statements are founded on what I saw in Glen Collarig, where the lower shelf (the 972 feet one) blends into a slope, now rendered irregular by the action of the torrents, which rises (at the gap) to a height of more than a hundred feet above the level of the shelf. Again, near the head of Lower Glen Roy, the seam shelf blends into a similar kind of plain, which rises (at the base of a terrace, projecting from the next shelf to it,) ninety feet (barometrically measured) above the level of that shelf to which it may be said to belong. In the east arm of Glen Turet, the upper shelves in a like manner terminate in slopes, which rise above their proper levels.

‡ Geological Transactions, vol. iv. (First Series), p. 320–338, and 387.

§ Edinburgh Royal Transactions, vol. ix. p. 12.

Sir LAUDER DICK has observed, and the fact is very important, that the head of Glen Gluoy is separated from the head of a branch of Glen Roy by a flat land-strait, with which the shelf in the former glen is exactly on a level; so that if Glen Gluoy were filled with water to the *full* level of its shelf, or a few inches above it, besides a great barrier at the lower end, a little mound, perhaps a foot or two in height, would be required to prevent the water flowing into Glen Roy. In the same manner if Glen Roy were closed at its lower end, and if water stood at the level of the upper shelf, it would trickle into the valley of the Spey. The same thing would happen with the lower shelf at the head of the valley of the Spean; and lastly, a short shelf, which I discovered in a gully, which enters the Caledonian Canal near Kilfinnin†, between

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* After the elaborate arguments given by MACCULLOCH, to show that no sudden rush of water, or debacle, could have formed the shelves, I should not have offered any remarks on this point, had not so distinguished a person as Sir GEORGE MACKENZIE (London and Edinburgh Philosophical Magazine, December 1835,) suggested such an hypothesis, without, however, it is fair to add, having visited the district. Each of the ten thousand pebbles, which together form any one buttress or little delta, and which, it is evident, were accumulated by the action of one streamlet, at the spot where it entered the expanse of ancient water,—each of these pebbles required time for its attrition,—each now plainly speaks against such an hypothesis.

† I was informed, but whether correctly I do not know, that the hamlet (in the middle of which there is a mound with a round tower on it,) on the opposite side of the valley, and a mile or two south of Invergarry, was named Kilfinnin. I therefore shall denominate the small stream which flows towards the Caledonian Canal at that point by this name. In the same manner I shall call the larger stream which debouches by Habercalder, and its valley, by that name, not having been able to learn any more proper one.

Loch Oich and Loch Lochy, is in a similar manner on a line with a peat moss, forming the watershed between it and another small valley. These four cases are so remarkable, that the coincidence of level must be intimately connected with the origin of the shelves; although such relation is not absolutely necessary, in as much as the middle shelf of Glen Roy is not on a level with any watershed. Sir Lauder endeavours to explain this fact by supposing that when the imaginary barriers of his separate lakes were perfect, the water flowed from that end of the glen, which is now highest, in other words, that the drainage of the supposed lakes was in each case in a reverse direction to that of the streams now occupying their beds. This view implies, moreover, the strange accident, that, during the breaking down of the barriers, the part that was originally lowest always remained standing, whilst a higher part gave way; and thus the removal of the barrier must be supposed to have happened from the effects of some causes no ways analogous to the wearing down of the mouths of lakes as they ordinarily exist.

The structure of these land-straits must be now described. This has already been minutely done by Sir Lauder with respect to that one which connects the sources of Glen Gluoy with those of Glen Turet, one of the arms of Glen Roy. The only additional observation which I have to make, is that the strait is broad and very level, and that on one side I noticed a beach, like that on a sea shore, of well-rounded pebbles. The accounts given by Macculloch and Sir Lauder of the division of the waters of Glen Roy and the Spey differ in some essential points. The latter author states that the upper shelf of Glen Roy is on a level (excluding the peat-moss) with the flat where the waters divide. This appears to be accurately the case, as far as the mountain barometer (which stood at the same thousandth of an inch on the two stations) and my eye could be trusted. But on the north side of the watershed there are patches of little terraces about fifteen feet above this level, resembling those which in other parts are connected with shelves, and hence probably having a similar origin with them. On the hill-side higher up, other obscure patches of alluvium occur with somewhat analogous forms. The water of the Spey first flows down a gentle mossy slope eastward, and is then collected in Loch Spey. On the south side of this loch, there is an obscure line of terrace, which appears to be about sixty feet above the loch, and which doubtless led Macculloch to suppose the upper shelf of Glen Roy was that number of feet above the division of water. The terrace above Loch Spey, as far as I could judge by the eye without a levelling instrument, is horizontal, and may perhaps be traced along the south side of the watershed, even a short distance within Upper Glen Roy, where certainly there occurs a mound parallel to and above the upper shelf. I much regret I was unavoidably prevented from examining this locality with all the attention it deserved. But from the structure of the small terraces, it appeared to me certain that water must for a period have occupied a level above that of the highest shelf of Glen Roy; and likewise that fragments of a shelf, or line of terrace, which as far as the eye could judge was horizontal, extended within the basin of the Spey, and therefore beyond the limits of the supposed lake of Roy. This latter fact is, at least, certain, for I have since learned, through the kindness of Sir David Brewster, that he has seen, as will be hereafter mentioned, shelves resembling those of Glen Roy at two points, at a distance of several miles down the valley of the Spey. The watershed at the head of the valley of Kilfinnin, has precisely the same character with the foregoing cases: here also a flat-topped buttress projects on one side above the level of the shelf, and this seems to indicate, as in the former case, the presence of water at a level rather above that of the shelf itself*.

The division of the waters between most of the glens and ravines in this district, in situations where no shelves occur, does not take place on a sharp ridge, but on level, and often broad land-straits, similar to those just described. I may instance a long one (at an elevation of between 1400 and 1500 feet above the sea,) separating two branches of the water which flows by Habercalder into the Great Glen, and one branch of the Tarf Water. Again another one nearer Fort Augustus, separating the two lowest and nearest branches of the same two rivers; here also there were obscure buttresses on each side above the level of the watershed. An intelligent shepherd who accompanied me, remarked that this form of the land was common wherever the waters in this mountainous country divided; and I observed several instances of it. Finally, I may remark, without wishing to lay any great stress on the argument, that these *land-straits*, whether connected with the shelves, or not, are precisely what might be expected from *straits*, properly so called, between arms of the sea being laid dry.

The discovery of any shelf, beyond the limits where they had been hitherto observed, being evidently an important point with regard to the theory of their origin, I shall fully describe the following case. At the head of a small stream which joins the Caledonian Canal, near Kilfinnin, and which is divided from the waters of the Habercalder by a flat mossy watershed already alluded to, some fragments of a shelf occur on the northern side. This shelf resembles in every respect those in Glen Roy; it seemed, as I walked along it, perfectly level, as it likewise did, when I viewed it from either end, and when I crossed the valley. I then took several measurements at the most distant points with the mountain barometer, and the mercury stood within the same hundredth of an inch. On the northern side of the valley, the shelf, which commences on a level with the mossy plain dividing the waters, extends for about a quarter of a mile almost continuously; it is then lost from the number of fragments of rock which have fallen down the hill, but reappears at the distance of more than half a mile from its commencement under the form of two or three little buttresses. These I ascertained by the barometer to be on a perfect level with the commencement of the shelf, or the watershed, a circumstance which was also apparent by the eye alone. The line further on disappears from the rockiness of the sides of the valley.

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* The pass of Muckul, described by Sir Lauder, which separates the waters of the Spean from a branch of the Spey, I did not visit.

On the south and opposite side of the valley, a broad sloping terrace extends at a corresponding level for about three quarters of a mile, but is indistinct owing to the gentle slope of the mountain. Further on it seems modelled into more than one terrace: and these, though obscure, appear to a person standing on them perfectly horizontal. Although the terraces are not plainly developed on this side, yet it is certain, that horizontal mounds, at nearly the level of the watershed, extend about two miles on the face of the mountain. With respect to the absolute elevation of this shelf, I made it about forty feet above the upper one of Glen Roy, and 1120 above Loch Lochy, or 1202 above the sea: but my barometrical observations have no pretensions to accuracy. After having observed this shelf from so many points of view, I am prepared positively to assert that it is in every respect as characteristic a shelf as any in Glen Roy; and although the fragments of it do not extend over more than, perhaps, half a mile in length, its origin must be as carefully attended to in any general theory of the formation of the shelves, as if its length had been twenty times as great. Its want of continuity and shortness possess, indeed, in themselves much interest, because we thus know that those causes which have marked with horizontal lines the sides of the mountains of Glen Roy in so wonderful a manner, have been in action here, though they have produced but little effect. Moreover, we see that if the surface had been originally rather more rocky, or had been less steeply inclined than at present, or had been subjected to a very little more alluvial action, all evidence would have been obliterated of the extension thus far of the action of these causes.

I have already alluded to the important fact communicated to me by Sir DAVID BREWSTER, namely, that he has seen shelves in the valley of the Spey. At Phones, which is situated about a mile from the Truim, and about five above its confluence with the Spey, one broad and well-marked shelf occurs, along which a carriage can be driven. On the banks of the Spey, about twenty-five miles below its source, two shelves occur in an elevated angle between the Burns of Belleville and the river. They are small; the upper one, however, is very broad; and their elevation is about 800 feet above the sea. Sir DAVID BREWSTER says, that the shelves in both places appear horizontal, and that they resemble those of Glen Roy, though possessing far less grandeur and symmetry. The fact of their occurrence at these distant points is, as we shall hereafter see, highly important.

Section II.—*The Theories of Sir* LAUDER DICK *and Dr.* MACCULLOCH *considered.*

Sir LAUDER believes that a separate lake existed in each valley, where we now see a shelf, and was separately drained. In Glen Roy, where three shelves occur, all plainly developed, (with the exception of Belleville, this is the only place where more than one has been observed,) the arguments in favour of a separate lake possess the greatest force. Without entering into any description of the physical features of Glen Roy, inspection of the accompanying map, taken with some few alterations from that of Sir Lauder Dick, will show the course of the shelves; although they cannot of course be followed nearly so continuously in nature as here represented. The lower one (972 feet above the sea) is common to nearly the whole line of the Spean and Glen Roy. The two upper shelves are confined to Glen Roy, with the exception of those short portions extending into Glen Collarig. It will be seen that both these lines, if continued round the hill of Bohuntine (at the eastern entrance of Glen Roy), would insulate it, whilst the lower shelf only forms it into a peninsula. From this structure it will be evident, that in order to form Glen Roy into a lake at either of the two upper levels, it would be necessary to erect two barriers, one across Glen Collarig, and the other principal one across the mouth of the Roy.

The lines are here represented as if abruptly cut off, but this is not so; and the following remark holds good in other cases, namely, that where a shelf terminates without any visible change in the nature of the slope, such as being rocky, &c., its disappearance is so extremely gradual, that it can be traced, sometimes to a further and sometimes to a lesser distance, according to the point from which it is viewed. Of this fact the shelves on the south-east side of Glen Collarig offer an excellent example. In the map, the extremities of the lower of the two upper shelves are represented at the four places where they terminate, as extending beyond those of the upper one. I state this on the authority of Sir Lauder Dick with respect to those in Glen Roy, and it is conspicuously the case with that pair in Glen Collarig which I have described as disappearing in so insensible a manner. The lower line can there be traced, though faintly, to a point below the houses of the glen opposite a small tributary torrent, and therefore considerably beyond (or nearer the mouth) than the point where the 972 feet shelf crosses the bottom of the valley. Observing in Glen Collarig the gradual disappearance of either set of lines, and that there is not the smallest apparent cause for it in the nature of the ground, the first and obvious supposition is that a sheet of water extended from the Spean into Glen Roy and Collarig, and that the mere widening of the mouths of the latter, as they approached the less protected expanse of the Spean, gradually became unfavourable to the accumulation of detritus, and therefore to the formation of the shelves. This view is greatly strengthened by the extension of the lower line in each case beyond the upper; for of course the supposed unfavourable condition for their formation, that is, the too great breadth and exposure of the sheet of water of which they formed the beach, would affect the line when the water stood at the higher level to a greater distance from the main expanse, or further up the valley, than when it occupied a lower level. It may, however, be argued (and on the hypothesis of Glen Roy having existed as a lake it must be so argued), that as the higher line is the oldest, so its terminal portion may soonest have yielded to those causes which modify the surface of the land. This view, however, receives little support from an examination of the rest of the glen, inasmuch as the two shelves through its whole course are in a state of equal preservation. We must therefore conclude, either that we now behold the shelves precisely as they were left by the sheet of water, or that if the two upper shelves did originally extend for an equal length on each side of the two glens, that the causes which tend in a small degree (for the existence of the shelves proves that no great changes have taken place) to smooth the surface, have acted over this district with the most *perfect uniformity*. Moreover, it may be remarked, that wherever a streamlet crosses a shelf, and it is probable from its size that it formerly delivered detritus to the ancient expanse of water, either a greater breadth of shelf or a small buttress there, attests that it was so; and in doing this, likewise attests how perfectly the surface of the land has been preserved. Now I paid particular attention to the following observation, namely, that on both sides of the hill of Bohuntine, and on the opposed mountains, where the shelves terminate, there was not the smallest change in the composition or in the outline of the smooth rounded surfaces. Yet it is in this very spot, where the lines insensibly disappear,—on these very hills, where the little deltas of the ancient streamlets are still preserved,—within this very district, where in the extension of the lower shelf beyond the upper one in the four cases, we have the most satisfactory proof of the action of absolutely uniform causes, either in their formation or in their obliteration; it is here, where the slope of the turf-covered hills is unbroken, where there is not a remnant of any projecting mass, that we are compelled by the theory to believe that the two enormous barriers stood, which formed Glen Roy into the imaginary Loch Roy.

But as it is highly important to show that such a Loch could not have existed, we must for a time, in the face of these great difficulties, suppose the two barriers to have been erected. It may be first remarked, that from the extension of the middle shelf, the barrier in Glen Collarig could not have occupied the only one place, which the structure of the ground indicates, even in the smallest degree, as probable, namely, at the Gap, where the waters divide; but it is necessary to suppose that it crossed the glen at a point some way distant from the Gap, and where the valley has a depth, below the upper shelf, of more than 300 feet. Glen Roy being now converted into a lake, with its drainage reversed, that is, with the water flowing from it by the Spey to the east coast of Scotland, let one of the two barriers, we will say the smaller one in Glen Collarig, give way from the effects of an earthquake or other cause. The lake will now stand at the level of the middle shelf, the barrier having given way eighty-two feet vertically. Again let it burst, and this time rather more than 212 feet vertical must be swept away, so that the larger lake, supposed by Sir LAUDER's hypothesis to occupy the valley of the Spean at the level of the 972 feet shelf, might send an arm a little way up the glen (as shown by the shelf now existing there) above the point where the barrier stood. Let all this have taken place, but still a barrier nearly a mile long, and 800 feet in height, is left standing across the mouth of the Roy. Must we suppose that each time the barrier in Glen Collarig failed, the one in Glen Roy gave way *the same number of feet* through some strange coincidence? or are we to conclude that some awful catastrophe at subsequent times, unconnected with the drainage of the lake, which must have passed through the breach already opened, removed the second barrier (either part or all of it) when *above water*, without having left the smallest remnant of it, or having disturbed the smooth alluvial covering of the steep slopes? The 972-feet shelf is common to the valley of the Spean and Glen Roy, and is supposed to have been formed by a lake, the barrier of which, some miles in length, extended near Highbridge across the mouth of the Spean. This shelf passes uninterruptedly, and with its usual breadth, on both sides of Glen Roy and of Glen Collarig, in the very part where the barriers of Loch Roy, if they existed, *must* have crossed the valley; therefore the whole, or part of the great base of those enormous barriers, must have been swept away when submerged within the bosom of the imaginary Loch Spean; and this must have been so perfectly effected, that no trace of them is left on the smooth slope of the hill, not even by a greater breadth of the shelf, any more than in the part of the second barrier, which must have been removed when above water*. And all this is supposed to have taken place on the hills, where I have shown how wonderfully the features of the land have been preserved, and where the boulders which were washed by the waves of the ancient water can be distinguished from those which have fallen since. In conclusion, therefore, I do not hesitate to affirm, that more convincing proofs of the non-existence of the imaginary Loch Roy could scarcely have been invented, with full play given to the imagination, than those which are marked in legible characters on the face of these hills†.

The same reasons which render the existence of a separate lake in Glen Roy so excessively improbable, apply with only little less force to each of the imaginary lakes in the other glens. We are, therefore, in giving up *Loch* Roy, involuntarily driven to the theory advanced by MACCULLOCH, namely, that all the valleys in which shelves occur were included in one large lake; but we shall thus run headlong even into greater difficulties. First, from the structure of the mountains, four immense barriers are required to form the lake‡, namely, one low down across the valley of the

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* I have not thought it worth while to enter into all the possible cases of this hypothesis, but have merely taken the most obvious one, which was assumed by Sir LAUDER. If any one has the boldness to come forward from the obscurity of past times, and state his belief that the broad barrier of the Spean was *erected* as well as removed altogether subsequently to the removal of the two barriers of Glen Roy, then the objection from the uniform breadth of the 972-feet shelf, where crossing the spot which must have been occupied by the barrier of Loch Roy, has less weight, but the other part of the argument remains valid. Again, on the hypothesis in the text, I have not entered into all the possible alternatives of the manner in which the bases of the Loch Roy barriers might have been removed, either when Loch Roy itself, or when Loch Spean was drained, or at some subsequent period by unknown causes connected with the drainage of the imaginary lakes.

† It should be remembered that it is far easier to assert than to disprove. If to explain some phenomenon it was stated that the Thames near London was formerly crossed by a barrier some hundred feet in height, of which it was not pretended a vestige now remained, it is difficult to imagine what kind of evidence would be sufficient to prove the hypothesis false, as long as any one was found willing to admit such an assumption.

‡ I may add, the same number of barriers are requisite, whether we suppose the existence of one, two, three,

Spey, two at distant points across the Great Glen of Scotland, and a fourth across the mouth of Loch Eil, the last being necessary, as MACCULLOCH shows*, from the structure of the Great Glen in that part. It may be safely asserted that more improbable situations could hardly be imagined in the whole of Scotland. It is perhaps useless to ask, were the barriers composed of rock or alluvium? if of the former, they were transverse to every line of hill in this part of the country; if of alluvium, we must assume an unexampled case; for where in the whole world shall we find even one barrier a mile and upward in length, and 1200 feet high, composed of loose waterworn materials? Secondly, the theory of one large lake does not explain in a satisfactory manner the remarkable coincidence between the shelves and the watersheds. Thirdly, when by the bursting of any one of the barriers, the level of the lake had fallen from one shelf to another, the hypothesis requires (as with *Loch* Roy) that the three other barriers, now high and dry, and distant many leagues from each other, should have been swept away by some unknown power, acting by some unknown and scarcely conceivable means, from the smooth sides of the mountains, without a remnant of them having been left; so that MACCULLOCH even frankly confesses one part is almost as probable (I would say improbable) as another for the position of the barriers. And it should be borne in mind, that these extraordinary forces are supposed to have acted on the outskirts of that large area, throughout which we have proofs, most wonderful and unequivocal, of the entire preservation of the surface of the land, as it was left at a period long anterior to the removal (if such removal ever did take place) of the barriers of the lower lakes. I do not hesitate to assert that this one difficulty, even by itself, would be sufficient to refute the theory of one great lake: Sir LAUDER's theory has been shown to be equally untenable. It is perhaps here almost superfluous to add, that the discovery of the shelf at Kilfinnin (and probably likewise of those in the valley of the Spey) increases every difficulty manifold; for the valley of Kilfinnin is almost as wide as it is long, which affects one theory, as the lowness of the opposite side of the Great Glen does equally the other. Finally, then, in giving up both, the conclusion is inevitable, that no hypothesis founded on the supposed existence of a sheet of water confined by *barriers*, that is, a lake, can be admitted as solving the problematical origin of the "parallel roads of Lochaber."

Section III.—*Proofs of the retreat of a body of water from the central parts of Scotland, and that this water was that of the sea.*

Having now discussed these views which cannot be admitted,—a method of reasoning always most unsatisfactory, but necessary in this instance from the high authority of those who have advanced them,—I will consider some other appearances,

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or as many lakes as glens; and the argument against MACCULLOCH's hypothesis of one lake, and against that of the separate lakes by Sir LAUDER, are applicable to any hypothesis requiring an intermediate number.

* Geological Transactions, vol. iv. p. 378.

which will perhaps throw light on the origin of the shelves. The valley of the Spean, from the point where it joins the Great Glen of Scotland to where it receives the Roy, is broad, and its bottom moderately level. The solid rock is concealed in almost every part, excepting where the river has cut itself a gorge, by irregularly horizontal strata of gravel, sand, and mud. Large portions of these beds have been removed along the centre of the valley, yet it is quite evident from the fringe or line of terraces which skirt each side, that the bottom must originally have formed a smooth concave surface inclined towards the mouth of the valley. Portions more or less perfect of this same deposit can be followed up the course of the Roy, and up the higher parts of the Spean, where the valley is not too rocky or narrow, to near Loch Laggan. This loch is but little below the 972-feet shelf; and at present I wish, for the sake of the independence of the argument derived from the facts to be stated, to consider only that part of the country which is below the level of that shelf. These irregularly stratified beds, near the mouth of the Spean, attain a thickness of several hundred feet, and they consist of sand and pebbles, many of the latter being perfectly waterworn. Higher up the valley, near the bridge of Roy, the thickness before the central portions were removed appears to have been about sixty feet, but of course the thickness varies according to the original irregularities of the rocky bottom of the valley. Now it may be asked by what agency has this sloping sheet of waterworn materials been deposited along the course of the valley? From the presence of the horizontal shelves we know that there has been no change in the relative level or inclination of the country since this district was last covered with water, and therefore we may argue with safety, that the action of the rivers, as far as it is determined by their inclination, must have been the same since that period as it now is, with the exception of that amount of change which they may have effected in their own beds. Our knowledge that there has been here no axis of elevation, with one part always rising a foot, and another a few inches less; but that the entire system of drainage has remained undisturbed and subject only to its own laws of change, is a circumstance which gives a singular degree of interest to the examination of this district. Now if we look at any portion of these rivers, for instance the Roy above its junction with the Spean, we find it has cut a narrow steep-sided gorge through the solid rock, which is in many parts between twenty and thirty feet deep, whilst on each side there are remnants, as above stated, of a continuous bed of gravel, at least sixty feet in thickness. These beds have certainly been deposited by rapid currents of water, but not by any overwhelming debacle, as may be inferred from the presence of cross layers, and the alternate ones of fine and coarse matter. Seeing also the evident relation of dimension and materials which exists between these deposits and the valleys in which they occur, it can scarcely be doubted that the detritus of which they are composed was transported by the existing rivers. But are we to suppose that the river, as in the case of the Roy, first deposited along its whole course these layers one over another, thus raising its bed sixty feet above the solid rock, and then suddenly commenced, without the smallest change in the inclination of the country, not only to remove the matter before deposited, but when having gained its former level, to act in a directly opposite manner, and to cut a deep channel in the living rock? Assuredly such a supposition will not be received; and whatever part the river had in the accumulation of these waterworn materials, from the very moment (neglecting the annual oscillations of action from the changing seasons) it ceased to add and began to remove, its power must have undergone some most important modification.

It will perhaps be thought that the mere deepening of the bed of the stream, near the mouth of the valley (the effect being slowly propagated upwards), could have caused the difference between the present and the former action of the river. But it is not difficult to replace in imagination the solid rock in the course of the Spean; and although a few small lakes will be thus formed, the average slope will not differ greatly from the present inclination, and this inclination we see is sufficient to cause the river to wear a deep gorge in the solid rock, and therefore it is evident (although I am aware that without actual measurement of the inclination this argument must rest upon eyesight, which cannot generally be trusted) that a change of this nature would be wholly insufficient to reverse the action of the river, as has here been the case. We must not, of course, at the same time replace in imagination those unconsolidated deposits, the origin of which we are considering; otherwise no doubt the inclination of the bed of the river would be greatly altered; although even in that case I by no means believe that the river would be so much retarded as to deposit matter at the heights where it is now left. Some check, therefore, to the transporting power of the stream seems to have acted at many, or at every successive level. If we reflect on what would result, as an hypothesis, from a river delivering during a long period detritus into a lake, the level of which was gradually sinking from the wearing down of its mouth, a gently sloping surface would be formed at its head. But as the barrier was cut deeper and deeper, and the lake sank, the stream in the part where it was once checked by meeting with the still water would gain velocity, and hence would cut through the beds which it had originally deposited. The fringe, of rudely stratified alluvium, the origin of which we are considering, resembles both in structure and composition such beds of detritus as would have accumulated on the shores of a lake, had one existed in these valleys. If, then, we suppose that a subsiding sheet of water did actually fill this valley, either of one or more lakes, with their barrier gradually wearing down, or of an arm of the sea, the general level of the ocean being stationary during a slow elevation of the land (as now is the case with the fiords of Scandinavia), every appearance on the sides of the valley of the Spean and Roy will be explained; and as there is no other way, that I can see, of accounting for them, the hypothesis is so far worthy of admission.

I ought, perhaps, to have previously observed, that these deposits could not have been formed when the valley was filled with water to the level of the shelves, for the detritus has the character of matter accumulated in shoal water, and the beds abut abruptly against the bases of the mountains, instead of blending with the alluvium on their surface, as would necessarily have happened had the whole been deposited at the same time at the bottom of one basin.

The conclusion, that these valleys have been occupied by a sheet of subsiding water, follows more plainly from a somewhat different class of facts. I have before remarked, that where a streamlet crosses a shelf, especially if it be the lower one, an obliquely truncated buttress, the form of which was represented by dotted lines in the wood-cut No. 1, projects from the side of the hill. It is quite evident that these were accumulated when the shelf existed as a beach, and the streamlet at present only acts in removing those portions with which it comes in contact. Now in some points where the buttresses have been somewhat largely developed, smaller ones at a lower level, composed of the same irregularly stratified waterworn materials, having nearly the same outline, although unconnected with any shelf, may be observed adhering to the slope of the hill. Instances of this structure occur on the east side of Glen Roy; on the south side of the Spean, and between Loch Treig and the bridge of Roy, the accumulation of perfectly rounded shingle, like that on a sea-beach, was enormous. The internal structure in this instance corresponded to the external form, as is shown in the accompanying diagram, where highly inclined beds of sand and coarse gravel are capped by other irregular ones of the same composition, only slightly inclined. In all these cases, where the flat-topped buttresses occur on steep slopes, it is certain (as might have been expected) that the streamlet is steadily at work in removing matter, and does not add one pebble to the mound. No one will dispute, that those buttresses, which are mere extensions of a line of shelf, were formed at the edge of an expanse of water (of which the shelf was the beach), and it is therefore by itself probable that the other buttresses, of similar external form and composition, though occurring at a different level, had a similar origin. But the argument may be put in a stronger point of view: taking the course of one of these streamlets, and observing the size and position relative to it of the buttresses one above the other, it becomes evident that the materials of which they are formed were accumulated through the agency of this stream, although it is at the same time inconceivable that they were left (especially in such a case as that represented in diagram 3.) on the steep slope by a power which, as it now acts, is steadily at work, tearing away matter in its whole downward course. Therefore, it is absolutely necessary to bring into play some intervening or modifying cause in the action of the streamlet; in the case of the buttresses which are connected with the shelves, no one can doubt what this intervening cause has been; shall we, then, rejecting a *vera causa*, seek some other one, if indeed such other can be found? Cer-

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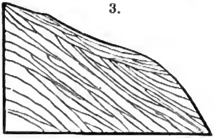

tainly not; and the conclusion is inevitable, namely, that a sheet of water must have stood at as many levels as there are buttresses, and this will include by short steps the whole space between the bottom of the valley and the lower shelf. Judging also from the amount of matter accumulated, we must infer that the water remained at these levels for no inconsiderable periods, although for a lesser time at each than at the level of the 972-feet shelf.

I would even further add, that in any valley (the relative level of the country, one part with another, having remained constant) a single buttress, if composed of such materials as could not have slided down the face of the hill in mass, or could not, judging by the presence of cross layers and alternations of fine and coarse beds, have been deposited by a debacle, indicates that the valley was once partly or entirely filled up to that height by such matter; and if the mass be too thick, or at too great an elevation on the sides of the valley, to allow of the supposition that it was deposited by the streams now flowing in the valley, subject to such changes in its velocity as by the corrosion of its own bed it could effect, then the formation of such buttresses can be accounted for only by the supposed permanence of a sheet of water, whether of a temporary lake or of the arm of the sea, at their levels. Now such projecting masses are extremely common in the sides of most of the tributary streams of the valleys. I conclude, therefore, from the consideration both of the beds of stratified alluvium at the bottom of the main valleys, which there is the greatest difficulty in believing could have been deposited by the rivers under the existing conditions; and of the buttresses on the sides of the hills, which similarly could not have been formed by the present streamlets, that it is satisfactorily proved that the valleys of the Spean and Roy have been occupied by a sheet of water which has slowly and very gradually retired, leaving in almost every part unequivocal evidence of the check which matter drifted by a current meets with, when it arrives at or near to the surface of still water.

I have as yet confined my argument to the valley of the Spean and its tributaries, and to that portion of it which is below the lower shelf; but I may here add, that it may be inferred from the same kind of evidence already used, (I allude more particularly to some buttresses above the 972-feet shelf to the north-east of the houses of Glen Turet, and to a shelf intermediate between the two upper ones, Tombhran,) that water long remained in Glen Roy at an altitude above that which we have as yet been considering, and at other levels besides those indicated by the three shelves themselves. If, also, we look to other valleys in this part of the country, we find similar appearances. For instance, on the flanks of the valley of the Tarf Water, which flows into Loch Ness (at the elevation of about 1000 feet, near the bridge, where the road to Garviemore crosses the river), there are large conical piles, with their summits truncated by a rude terrace, composed of well-rounded pebbles, sand, and an argillaceous earth in irregular beds. Some of the layers of sand and fine gravel were in curves, but slightly inclined; and this structure, together with their composition, made it at once evident that they must have been drifted into their present position by currents of water. Again, near Fort Augustus, the Great Glen, with the exception of the central part, where the river has worn for itself a broad course, is filled with irregular strata about thirty feet in thickness of sand, gravel, and coarse shingle. In the sand some of the layers are most regularly waved, as if by a tide ripple. These beds are about seventy feet above the sea; fringes of similar deposits skirt at intervals both sides of the Great Glen, but where they are present they do not occur, as far as I was enabled to observe, at a greater height than about 100 feet, that is, than the water-shed of this great valley,—a fact somewhat analogous to the coincidence in level between the true shelves or roads and the heads of the valleys in which they occur. At the south-west end of the Great Glen, nearly opposite to Loch Leven, there are some extensive flats, which from a distance appear to be similarly composed, and which in one part have been modeled into two nearly regular terraces, one rising above the other. A somewhat similar structure may be observed in a part between Loch Eil and Loch Lochy; and this structure can only be explained by water having successively occupied for long periods different levels.

Referring now to more distant points, we find in the broad valley below Loch Tulla (a tributary of Loch Awe, and the stream flowing thence enters Loch Etive,) there are some appearances, although obscure, of the bottom of the valley having been once filled up with stratified alluvium. On the river Tay, however, near Loch Dochart, the phenomenon is clearly developed. On the south side there is a long mound or terrace, about 150 feet high, entirely composed of well-rounded pebbles mingled in layers with a yellow sandy clay. From this point to Tyndrum (at an elevation of between 400 and 500 feet above the sea) there are similar banks of waterworn materials, and in more than one part I observed a fine white sand, like that on the seashore. On each side of the valley where it divides, near Tyndrum, a broad expanse is scattered over with low ridges and flat-topped hills of equal height, from which it would appear that the whole space had once been covered up with these deposits. Towards the mouth of the Tay the terraces and platforms of Strathmore have been remarked by many observers, on the sides of the small neighbouring valley of the Dighty. Mr. BLACKADDER, in a letter to Mr. LYELL, says, "A narrow track of gravel, sometimes in the shape of platforms, at others in small hillocks, very similar in appearance to those of Strathmore, extends to the height of about 600 feet; and some isolated patches on the southern face of the Sidlaw Hills occur at a greater elevation." From expressions used by MACCULLOCH and other writers, I am led to believe that beds of similar matter irregularly superimposed over each other, occur on the sides of almost all the valleys of Scotland. In such cases as in that of Loch Dochart, we have no proofs, as horizontal shelves or ancient beaches have not been preserved, that the relative level of the country has remained the same, since the period when it was first traversed by running streams; and therefore it is not absolutely certain that the present rivers, with a very different inclination, might not have deposited the rudely stratified beds in the lower part of their courses, and afterwards with an altered velocity have cut through them. But as we do know that no such change has affected a large neighbouring region, and as such movements could hardly thus have influenced the drainage of valleys directed towards different quarters, such doubts may be overruled. This being the case, the same argument as before used may be repeated, namely, that the waterworn materials appear to have been transported by the present rivers, and yet that they are so deposited as could not have happened without some intervening cause. The phenomenon demands an explanation; and the only obvious solution is that which from several and nearly independent considerations was proved to have been the case with the Spean, namely, that it had been occupied by an expanse of gradually subsiding water, either of a lake or of an arm of the sea. This conclusion, therefore, may be urged with only little less force regarding many, if not all, of the valleys in this part of Scotland.

It may be asked, of what nature was this sheet of water? If we suppose a barrier erected across the mouth of each valley, and a lake to be thus formed, which sunk from the gradual deepening of its mouth, all the appearances above described would be explained. It is a startling assumption to close up the mouth of even one valley by an enormous imaginary barrier; to do this with all would be monstrous. Of such barriers in the district we are considering I need not say there does not exist any trace, nor need I repeat what I have already said against so vain a supposition as that they could have been swept away by any great debacle from the sides of those hills, of which the whole alluvial covering has been preserved since the period when the upper shelves formed beaches, without even a remnant of them being left; and I may add, that it will hereafter be shown by the clearest proofs, that the ordinary alluvial action, and likewise that of running water, even under the most favourable circumstances of a waterfall, has been far less efficient than could have been anticipated.

But it may be asked, would not the hypothesis of a succession of lakes explain the appearance, the matter accumulated above each delta sloping upwards from one level to another. I can only answer this with respect to those valleys which I have myself seen: in the Spean, Roy, Tarf Water, and some others, it is easy, as before stated, to replace in imagination the solid rock; and although some small lakes* would be thus formed by the replaced barriers (as probably would be the case in every valley), the fringe of stratified alluvium we are now speaking of skirts the valley at an elevation above them. To assume that these rocky barriers were formerly much higher, and were demolished by some means independent of the action of the river (for this action tends only to form a narrow wall-sided gorge, as may be seen in those barriers which certainly did exist), would be as gratuitous as the imaginary erection of one great barrier across the mouth of the valley, and would explain, from the continuity of the slope, the appearances far less perfectly. Moreover, if the origin of the sloping fringes could be explained by the assumed former existence of a chain of lakes, the buttresses high up on the sides of the valleys clearly could not be so. Nor will any one pretend that any lake-theory can be applicable to the deposits on the sides of the great valleys, such as Strathmore, and the Great Glen of Scotland, which terminate in deep and open friths. Therefore it has not been the water of several lakes any more than of one lake, which slowly retiring from these valleys, determined the accumulation of the beds, where we now see them. There is, then, as we have conclusive evidence that an expanse of slowly subsiding water did occupy these spaces, but one alternative, which we are compelled to admit, and this without any consideration of the *shelves* themselves, excepting so far as they serve as artificial levels to show that the country has not been unequally elevated, namely, that the waters of the sea, in the form of narrow arms or lochs, such as those now deeply penetrating the western coast, once entered and gradually retired from these several valleys.

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* Sir LAUDER has represented three in his map (Edinburgh Royal Transactions) by the figures 5, 6, and 7. I cannot, however, by any means agree with him in the limits thus assigned to them. Is it meant to be asserted, that there is any barrier perfect, with the exception of such a gorge as the river is now cutting, at the lower end of number (7), on a level with the line at its upper extremity; or so nearly so as to allow of the upper part being considered as a supralittoral delta? Such did not by any means appear to me to be the case. Was not the barrier only supposed to have existed, as in the theory of the shelves? I must also observe that the fringe or deposit does not terminate a little way within the mouth of the Roy, as represented by the line marked (7). It appears to me unfortunate that Sir LAUDER marked the limits of these deposits, which are accumulated in a *gentle slope*, in a similar manner as he has done the shelves, which are *horizontal*. Any one would suppose the lines 5, 6, and 7 were horizontal, like those marked 1, 2, 3, and 4. This difference alone indicates a corresponding one in their origin, as will hereafter be attempted to be shown.

Section IV.—*Proofs from organic remains of a change of level between the land and the sea in Scotland. The effects of elevation traced in hypothesis.*

Another question immediately arises; did the waters of the sea slowly subside, or the land slowly rise, the effect in each case being similar? But first it will be proper to show, from the more ordinary kind of evidence, that there has been some change of level between land and water affecting Scotland within recent times, although not to the amount inferred from the arguments above advanced. Mr. Smith of Jordanhill, in an excellent paper*, has lately shown from the presence of elevated organic remains, that within a period geologically extremely recent, both the east and west coast of Scotland has been raised some hundred feet; namely, at Banff and near Glasgow† about 350 feet. Considering the facts given in this paper, it can scarcely be doubted, without making the most improbable assumptions, that the Great Glen of Scotland, of which the highest point is only ninety-three feet above the sea, was within this recent period an open strait; and, I may add, it must then have strikingly resembled the Beagle Channel in Tierra del Fuego, an arm of the sea narrower, longer, and straighter, which intersects the extreme southern part of South America. In accordance with this fact, I was informed by the person who now has the charge of the locks on the canal, that when they were cutting through the gravel at the head of Loch Ness many broken sea shells were found in the *lower* part, which appeared to him like those on the sea-coast. When exposed to the atmosphere they soon decayed. This point must be between forty and fifty feet above the level of the sea. There are remnants, as before stated, in this part of the Great Glen, as well as at the south-west extremity, of coarse sublittoral formations, which, I suppose scarcely any one would dispute, were accumulated before that small change of level took place, which is indicated by the elevated marine remains. That the movement must have been exceedingly slow, may be inferred from the existence of so many beaches, each requiring time for its formation, which rise one above another on both coasts of Scotland. Mr. MALCOLMSON* mentions no less than eleven in Elgin, from the lower one of which he procured twelve species of existing marine Testacea. On the opposite coast also, Mr. Smith has described† several ancient beaches between the present one, and the great terrace, between thirty and forty feet high, which "forms a marked feature in the scenery of the west of Scotland." It is also important to observe here, that the supposed greater movement deduced from the nature of the superficial deposits, is of precisely the same slow kind, and interrupted (as will presently be shown) by periods of rest, as this lesser movement, attested by the presence of sea shells and step-formed beaches. If, then, the Great Glen was for a long period occupied by an arm of the sea, which very slowly retired from it, deposits must have accumulated on its shores, and likewise for some little distance within the mouths of the valleys which entered it. If we suppose that the sea stood at the same level in the Great Glen as it lately did both on the east and west coast, then the salt water would have almost entered Glen Roy, and would have wholly covered that sloping fringe of gravel, which has been so often mentioned as skirting the course of the Spean. Whether this be granted or not, after what has been stated it can hardly be disputed, that within recent geological periods an arm of the sea entered at least the mouth of the Spean, and very slowly retreated from it. Remembering that the conclusion was forced on us by distinct lines of arguments, that a body of water must have slowly retired from these valleys, and that lakes sufficiently large to have produced the observed effects could not have existed in them, may we not, with the additional consideration that some parts of the deposits here *must* be of marine origin, deliberately affirm it proved, that it was the waters of the sea that, even at great heights, checked and banked up at successive levels, the detritus brought down by the ancient rivers and streamlets? I am aware that the argument would have had a greater *appearance* of strength had I commenced with the inference deduced from the presence of recent shells at considerable elevations on both coasts of this kingdom, but I preferred the method I have followed, because I believe it is equally legitimate, and of more general application, although at first not so obvious.

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* Edinburgh New Philosophical Journal, vol. xxv. p. 376.

† Edinburgh New Philosophical Journal, vol. xxv. p. 386 and 387. The elevated shells at Banff were observed by Mr. Prestwich, Proceedings of Geological Society, May, 1837.

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* Proceedings of the Geological Society, 1838, p. 669. I was informed by an intelligent quarryman that he had observed many broken sea shells in a gravel-pit, about two miles north of Grant Town, on the roadside to Forres, and therefore eighteen miles from the nearest sea-coast.

† Edinburgh Philosophical Journal, p. 388.

From these facts it is certain that there has been a change of level affecting within recent times the whole central part of Scotland, and of a kind very similar to that which has been the subject of so much attention in Sweden, where, according to Mr. Lyell, remains of existing marine animals have been raised to the height of between 500 and 600 feet above the sea. The change of level in the case of Sweden is as certainly known to be due to a slow movement of the land, and not of the water, as it is on the coast of Chile, where a small tract is violently upraised during an earthquake, the distant parts of the same coast being unmoved. It would, however, be quite superfluous here to enter into this question at length, as it has almost ceased to be debateable ground*. It may then be concluded that the supposed great change of level in Scotland, deduced from the foregoing arguments, as well as that smaller fraction of it attested by marine remains and ancient sea-beaches, is due to the rising of the land, and not to the sinking of the waters.

We will now endeavour to trace in hypothesis the effects which would be produced by an arm of the sea slowly retiring from inlets during an *equably progressive* elevation of the land. In a deserted sound or flat-bottomed valley, surrounded by mountains, curved lines crossing the river would mark the ancient beaches. Each of these lines would be higher than its neighbour on the sea-side, owing to the rising of the land in the interval of their formation, and would be more distant from the head of the valley, chiefly on account of the matter brought down by the river, and in some parts from the natural slope of the fundamental rock. When the upper line formed a beach, it is evident that the whole of the lower part of the valley must have been under water, and that the prolongation of the beach would stretch along the flanks of the adjoining mountains some way inland from the present shore. In like manner each successive and lower beach-line would wind along the steep sides of the hills, and cross the valley further and further from its head. It should be observed, that although I have spoken of successive beach-lines, yet as the land is supposed by the hypothesis to rise at a perfectly equal rate, every part of the valley will have successively formed, during an equal period, a beach; so that each part having been similarly exposed, the slope will be uniform; nor will it be possible to distinguish any one line of beach. Again, if we suppose matter to be removed from the valley by the action of the tides, instead of being added to it by the river, yet as an equal quantity (or a quantity insensibly varying from the varying degree of exposure, as the form of the land slowly changes during its rise) would be removed at each level, the slope in this case also would be uniform. In that part of each successive beach, which winds along the steep flanks of the mountains, it is not probable that much matter would be added, but the downward descent of some portion of the detritus, which is formed on all land by meteoric agency, would be checked; but as it would be equally checked at each successive level, the outline of the mountain would remain unbroken. These same lines, however, although protected in the more inland parts, might suffer degradation where exposed to the greater force of the waves near the mouth of the sound; but the parts differently affected would blend into each other, and so would it be with each successive beach-line; and the slope therefore, whether added to or corroded, or left untouched, would never show the traces of action on any one defined horizontal line. A little reflection will indeed show that when the water stood at the highest level, any part or point which happened to be most exposed would, from the natural slope of all mountains, be some way inland compared with the same relative point on the present coast; at all intermediate levels the waves would attack an intermediate part, either high up and more inland, or lower down and nearer the coast, so that the line (or rather zone) of greatest littoral action, joining the parts which were successively most affected, would, under the conditions of the hypothesis, be inclined with the horizon either more or less, according to the original inclination of the land. Lastly, the river in the valley, as it gained power from the sinking of the sea, would generally remove the central portions, and leave only a fringe of the littoral and sublittoral deposits. This fringe, although formed by successive horizontal beach-lines, would *slope* upwards, as the whole bottom of the valley would have done if no part had been removed. I allude to this structure more particularly, because it is not at first obvious that matter accumulated on a sea-shore would in any case form a fringe of this kind.

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* An excellent summary of the argument is given by Mr. Lyell in his Elements of Geology, chap. v.

In the hypothesis I have supposed the upward movement of the earth to have been absolutely uniform during equal periods. But this probably has seldom been the course of nature. There is clear evidence that the action of volcanos is intermittent; and the force which keeps volcanos in action being absolutely the same with that which elevates continents (as I endeavoured to prove in a paper read not long since, March 7th, 1838, before the Geological Society), so we must suppose that the elevation of continents is likewise intermittent,—a conclusion which receives ample confirmation from the occurrence in nature of successive lines of escarpment, rising one above another, which mark those periods of rest when the sea wore deeply into the former coast. Let us then suppose that the water stood for a longer time at some one level than at any other. The first effect would be, that the beach or delta at the head of the sound, where the river is constantly bringing down detritus, would be broader there, owing to the greater accumulation of matter during this longer period, than in any other part; and therefore when the bottom of the whole valley was converted into land, the slope, which is everywhere gentle, would in that part approach nearer to horizontality; but in other respects there would be scarcely any difference. In like manner, in those portions of the mountains, on each side of the valley, where from the protected nature of the site matter did during the whole rise accumulate, though very slowly, the line would, from the greater quantity of matter added during the longer period of rest, slightly project beyond the general slope of the surface; and where any rivulet came down a very little delta would be formed. Also on any projecting or exposed point, the solid rock would be more deeply cut into than in the other lines. But as the land rose, the little deltas gently sloping from the line of ancient beach, with their front part cut off by the action of the subsiding waters, would project from the hill sides in the form of obliquely truncated buttresses; to the heads of which the horizontal lines of beach will exactly coincide, as indeed they likewise will with the broader ones, where crossing the bottom of the main valleys; but the slope in the latter case will blend both above and below with the inclined surface formed by the matter rapidly accumulated at every successive level. Now it has been shown that Scotland within modern times has undergone a great elevation; it has been shown to be extremely improbable that such movements should be equally progressive: the effects of aqueous action on the surface of the land during the intermittent periods of rest in the elevatory forces have been traced; and it will have been perceived by those who have read the early part of this paper, or the memoirs of Sir Lauder Dick and Dr. Macculloch, that the results anticipated in the hypothesis are the characteristic features, even in detail, of the "parallel roads of Lochaber": I believe, then, that the hypothetical case gives the true theory of their origin.

Section V.—*Objections to the theory from the non-extension of the shelves, and the absence of organic remains at great heights, answered.*

Several objections to this view, which implies that the whole country has been slowly elevated, the movements having been interrupted by as many periods of rest as there are shelves, will occur to every one. Perhaps the most important of these is, that, as the upward movement probably affected a considerable area, or at least as it cannot be supposed to have been confined within a defined line, so ought the shelves to be continuous over an equal space. I believe, however, from what I have seen in South America, that it would be more proper to consider the preservation of these ancient beaches as the anomaly, and their obliteration from meteoric agency the ordinary course of nature. Some contingencies seem absolutely necessary for the formation of the shelves, such as a sufficient height in the land, a steep slope, and that the country should be formed of rocks which afforded an abundance of somewhat adhesive detritus; we may conclude, moreover, that the surface must have been covered with turf, *immediately* after the waters subsided; for otherwise the loose matter would infallibly have been washed from the hills, and this contingency implies a protected, and hence, perhaps, an *inland* situation, which, at the period, when the water stood at the upper shelves, would leave but a small area. The abundance of detritus no doubt is quite necessary; for although the solid rock is in some parts notched, I do not believe the shelf would anywhere be distinguishable if the soil and detritus were entirely removed from it. It would also appear to be necessary that the valley should either have been originally closed at its upper end, or that during the period of rest some shallow part in it should have become so from the accumulation of sediment, or from any other cause, so that no stream set through it. Thus the two upper shelves of Glen Roy die away as soon as they enter the valley of the Spean, which must at the period when the waters stood at their levels, have formed an open channel connecting opposite seas. That the ancient beaches in this case extended to that point, beyond which the accumulation of matter was prevented by too much exposure, seems clearly indicated, in a manner before explained, by the extremities of the lower shelf stretching beyond those of the upper. When, however, the 972-feet shelf existed as a beach, the channel of the Spean was converted by the closing of the pass of Muckul into a sound; and the shelf, apparently in consequence, winds along the sides of the valley both of the Spean and Roy. Besides the requisites here mentioned, the shelves appear to be more plainly marked where the valley is narrow, and, perhaps, likewise where it is tortuous. Now from the little I have seen of Scotland, I very much doubt whether these several contingencies occur frequently together; they certainly did not in several valleys which I visited. It must also be borne in mind, that as Sir Lauder Dick traced the lower shelf very much further than Macculloch had done, and as I found a remnant of one in a distinct valley, and especially as Sir David Brewster has seen shelves in two places on the Spey, the probability is that others, though perhaps obscurely developed, will yet be discovered. The irregularly shaped area, in which shelves have already been found, measures in one line twenty British miles, and in another twenty-five.

Notwithstanding what I have now said, the presence of the shelves in some of the glens and their absence in others, in the district of Lochaber itself, is a very extraordinary circumstance. Thus in Glen Roy three lines are perfectly developed, whilst in the neighbouring one of Glen Gluoy it appears that only one exists. It is useless without data to speculate on the nature and force of the tides, currents, and winds of former periods, or on the kind of vegetation with which the land was then covered; all circumstances, perhaps, sufficient to determine the formation or preservation of a mere narrow mound of soft matter on the steep side of a mountain. But the following case proves, and it deserves particular attention, that the limits of the ancient waters cannot even approximately be inferred from the present extension of the ancient beach-lines. Macculloch has drawn in his map a shelf intermediate between the two upper ones, on the face of the mountain (Tombhran) opposite to where Glen Turet joins Glen Roy: Sir Lauder Dick has not noticed this shelf*. Perceiving its importance I examined it with scrupulous care. It occurs rather nearer the lower than upper shelf, and as these two are only eighty-two feet apart, and are here strongly marked, it was scarcely possible (especially as I purposely looked at it from *every* point of view,) to make any mistake in the absolute parallelism of this intermediate shelf. It can be traced for nearly three quarters of a mile; at the west end disappearing quite insensibly, like the lines in Glen Collarig, but at the other end rather more abruptly in a water course. I walked along its whole length, and its structure is perfectly characteristic; I refer to the materials of which it is composed, its breadth and inclination. The two regular shelves are, perhaps, more plainly marked here than in any other part of the whole glen; and it would appear probable that this is owing to that portion having been exposed to a longer space of open water, by which means the ancient waves acquired a greater than ordinary power in heaping up detritus. In the mouth, however, of Glen Collarig and of Glen Roy, an exposure to a wider channel, but at the same time to one open at both ends, and therefore probably a tide-way, has entirely prevented the accumulation of matter; and hence the beaches gradually disappear there. This view, if correct, as I fully believe it to be, shows by what a slight difference of circumstances, either a remarkable development or an entire obliteration of the ancient beaches has been determined. The intermediate shelf clearly owes its existence to the same causes which have in this part so strongly marked the upper and lower one; and though it is less strongly marked than these two in this immediate neighbourhood, yet it differs but little from them as they ordinarily occur, and is, I think, fully as plain as the lower shelf throughout Glen Spean. I assert, then, that it is an incontestable fact, that water must have remained at the level of this intermediate shelf for a long period, and only a little less long than at the other lines; yet in no other part of Glen Roy, the valley where circumstances have been so pre-eminently favourable for the formation and preservation of these beaches, a trace of this intermediate shelf has been observed. It has likewise been most clearly shown, that barriers could not have existed at the double mouth of Glen Roy, and we have seen that the surface of the land has been preserved in that neighbourhood in a manner quite extraordinary; yet it is known on the authority of Sir LAUDER DICK, who appears to have examined the whole course of the Spean and its tributaries with great care, that not a vestige of either of these upper shelves can be discovered beyond the mouths of Glen Roy. Any argument, therefore, whatever, from the non-existence of the shelves or beaches bearing on the former limits of the ocean over this part of Scotland, during the period of rest in the subterranean movements, is valueless.

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* Until I saw this shelf I doubted its existence, because I had not been able to discover others mentioned by Macculloch: thus one is figured by him in a ravine branching from Glen Roy (improperly called by him Glen Fintec), which, though having ascended it, I was unable to see. Again Macculloch states, that two shelves occur in Glen Gluoy, whilst Sir Lauder Dick, who seems to have examined most carefully this glen, could find only one. I may here remark, that should two shelves be hereafter discovered there at the same relative height from each other with those of Glen Roy, and this is stated to be the case by Macculloch, the fact would be highly satisfactory on the theory of the shelves having been sea-beaches. From an excellent point of view, however, on the side of Ben Erin I could see no trace of a second shelf. Macculloch also

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figures a supernumerary shelf at a point north-west of the houses of Glen Turet, at a level above that of the upper shelf of Glen Roy; a mound of alluvium, above, and nearly parallel to the shelf, certainly occurs there; but from the want of sharpness of outline, I should be unwilling to pronounce that it had formed a line of beach, although I should be far from feeling any surprise if this could be shown to have been the case.

In the valleys of the Spean and the Roy, I attentively examined, with the expectation of finding fragments of sea shells, the matter accumulated on the shelves, and more especially the thicker beds of gravel and sand which occur at lower levels; but I could not discover a particle, and the quarrymen assured me they had never observed any. This may at first be thought a strong objection against the theory of the marine origin of these deposits. But having been led in consequence of Mr. MURCHISON's remarkable discovery of recent sea shells in the inland counties of Shropshire and Staffordshire, to examine many gravel pits there, and having observed how frequently it happens, that not the smallest particle can be discovered in vast accumulations of the rudely stratified matter, and that when found, the fragments are generally exceedingly few in number and partially decayed, I feel convinced that their preservation may be considered as a remarkable and not as an ordinary circumstance. After a longer interval of time, or under some slightly less favourable conditions, all the gravel beds of Shropshire, which no one can doubt were accumulated beneath the sea, would be as destitute of organic remains as those of Lochaber. In some parts of South America I have found beds of gravel which did not contain a fragment of shell, and yet on the bare surface, nearly perfect ones were strewed in numbers. Mr. SMITH describes[*] beds on the west coast of Scotland, and Mr. LYELL[†] others in Sweden, undoubtedly of marine origin, but wholly destitute of organic remains. On the coast of Forfarshire also Mr. LYELL, as I am informed by him, found shells in gravel beds extending to the height of between fifty and sixty feet; but at greater altitudes similar beds occur which do not contain any: he has observed the same kind of fact strikingly illustrated in Norway[‡]. It is easy to imagine several

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[*] Edinburgh New Philosophical Journal, vol. xxv. p. 380.

[†] Transactions of the Royal Society, 1836, p. 11. and 15.

[‡] Mr. LYELL has had the kindness to give me the following observations on this point.

"In the country surrounding the fiord of Christiania, especially between Christiania and Dramman, and between Dramman and Holmstrand in Norway, deposits of clay and sand rest in horizontal beds on the gneiss, granite, porphyry, and other rocks. Large masses of this sand and clay reach in some places to elevations of more than 600 feet above the level of the sea, and nearly fill many upland valleys; but it is only in those patches which occur at the height of about 200 feet, and usually less than fifty feet above the sea, that shells (all of recent species) have been found. This sand and clay appear to have accumulated on the older rocks during their gradual upheaval from beneath the sea, so that greater elevation becomes a test of higher antiquity, and those patches which are found at small heights near the borders of the present fiord are very modern. Even in these last the shells are often in so advanced a state of decomposition as greatly to favour the theory that a more considerable lapse of time might be sufficient to obliterate all traces of their existence. Thus for example, on the banks of a small river about two miles above Töusberg at the place where the bridge crosses it, a section of loamy clay is laid open, the lowest part of which cannot be raised more than a few feet above the salt water of the fiord of Christiania. In the upper part of the mass for a thickness of fifteen feet no fossils can be detected, but somewhat lower faint casts of the *Mytilus edulis*, chiefly indicated by purple stains, are observable. Still lower down more perfect specimens of the same shell, together with *Cardium edule*, occur, but both in so soft a state as to crumble into dust when dried. With these the more solid *Cyprina islandica* and *Saxicava rugosa* are occasionally found, and although soft when first taken from the matrix are capable when dried of being preserved entire. If in the short period which has probably passed away since these shells circumstances which might determine the preservation or decay of the shells; even on the assumption, which is not necessary, that they have in all such cases been imbedded. Thus in Shropshire, the gravel is covered in most parts by an earthy deposit, which contains a small proportion of lime; hence the rain water having absorbed carbonic acid gas in its descent, would find matter to dissolve before it reached the layers containing shells; whereas in Lochaber the gravel and sand, being derived entirely from granite rocks, does not, as I ascertained, usually contain any free carbonate of lime, and consequently the fragments of shells would more readily be dissolved. I do not wish to assign this circumstance* as the real cause of their disappearance, but merely to indicate it, and other similar ones, as quite sufficient to show that the marine origin of the shelves cannot be controverted from the absence of organic remains.

Section VI.—*Application of the theory to some less important points of structure in the district of Lochaber, and recapitulation.*

By considering the hypothetical case above given, I think it was shown that the proposed theory explains every essential point in the phenomenon of the parallel roads. And I will now endeavour to show how far it applies to some minor points of detail. For instance, I have described a horizontal band of rock on one side of the narrow mouth of Loch Treig, with its face worn into smooth concave forms, like those over which a water-fall rushes; and on the other side, a great spit or bank of sand and gravel. Now on the belief, that a sheet of water seven or eight miles long, and two or three broad, was drained during each ebb-tide to the depth of several feet through a narrow curved channel, and then again raised by the following tide to its former level, the effects there produced are quite intelligible. It is also easy to perceive, that through the means of the tidal action, points of solid rock might have been obliquely cut off in the same manner as on existing beaches; and that flat channels, resembling in every respect those which at present frequently separate small

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near Tönsberg were imbedded, the progress of decay can have proceeded so far, we may well suppose the percolation of water during antecedent ages of indefinite extent to have destroyed all signs of fossils in the more ancient and elevated patches of loam found more than 500 feet high in the adjacent hilly country."

* I may observe that it very frequently happens, that shells are found only at some depths in these superficial deposits: this is the case in several of the gravel pits in Shropshire; in cutting the canal at the head of Loch Ness, the shells were met with at the bottom, whereas, the layers nearer the surface, as I can vouch, contain none. Mr. SMITH speaking (p. 380 and 391. vol. xxv. Philosophical Journal,) of the clay beds on the west coast of Scotland, says, that the marine remains with which it abounds "are almost invariably found in the lower part of the bed." I infer that in all these cases shells originally existed in the upper part, but have since decayed: Mr. SMITH, however, offers a different explanation. In the extensive and superficial beds of elevated shells on the coast of Peru, where rain does not fall, and where consequently loose matter is not washed from the surface, I have traced as I have ascended from the beach a most perfect gradation in the decay of the shells, until a mere layer of calcareous powder, without a vestige of structure, alone remained.

islands from larger ones, might have been worn between hummocks (such as those on one side of Meal-derry) and the lines of shelf.

If, again, we consider what must take place during the gradual rise of a group of islands, we shall have the currents endeavouring to cut down and deepen some shallow parts in the channels, as they are successively brought near the surface, but tending from the opposition of tides to choke up others with littoral deposits. During a long interval of rest in the upward movements, from the length of time allowed to the above processes, which essentially require time (though they are favoured by the *rise* of the land rather than by its remaining stationary), the tendency would often prove effective both in forming by accumulation of matter, isthmuses, and in keeping open channels. Hence such isthmuses and channels just kept open, would oftener be formed at the level, which the waters held during the interval of rest, than at any one other. These isthmuses and channels when left by the receding waves, might be called land-straits, for they would present smooth, flat, narrow surfaces, connecting more open spaces. During the rise of the land they would at first separate the heads of two adjoining creeks, and afterwards, the upward movements proceeding, they would form the watersheds between adjoining and opposite glens. By this means, I explain both the ordinary structure of the land in these mountains, where the waters divide, as already described; and more especially the remarkable fact of the exact coincidence of several such points with the lines of shelves,—the shelves only indicating the long interval of rest in the upward subterranean movements. It may be remembered that I described at the head of the Roy and of the glen near Kilfinnin, patches of alluvium or remnants of terraces on the sides of the land-straits, a little above the flat where the waters divide. This structure is in perfect accordance with the theory that drift matter began to accumulate in such parts at that period, when the tides in them were first checked, or otherwise affected by the rising of the land; and that the channels were finally closed at their present levels, solely from the long interval during which the sea acted at such levels. Hence, also, we might have expected, that patches of alluvium would occur (as is the case) on the sides both of the land-straits which are, and those which are not connected with shelves at corresponding levels.

From the levels taken by Mr. MACLEAN with Sir LAUDER DICK, it appears that the upper limit of the Glen Gluoy shelf, which coincides with the division of the waters, is twelve feet higher than that of Glen Roy. The intervening space is nearly a mile in length, moderately broad, and very flat, having only a fall of the twelve feet; and Sir LAUDER states* that he saw in this part the surface of the solid rock in the bed of the little stream. These facts seem at first to indicate that two periods of rest had supervened, one when the water stood at the level of the Glen Gluoy shelf, a second when at the upper level of Glen Roy after a rise of twelve feet, and that, nevertheless, the effects of these two periods of rest were confined respectively to separate, though closely adjoining glens. This circumstance if so interpreted, although improbable in the highest degree, could not be considered as subversive of the theory, after it has been ascertained that the upper shelves of Glen Roy are not prolonged into the valley of the Spean, and that the short intermediate one in Glen Roy does not extend for more than three quarters of a mile in that valley. There is, however, I suspect a more satisfactory explanation. In the First Narrow of the Strait of Magellan, the tide rises about forty feet, as Captain FITZROY informs me, whilst eighteen miles to the west at Gregory Bay, the rise is only about twenty feet. Here then, and other instances might be adduced, in a distance of eighteen miles, the surface of the water must slope no less than twenty feet. Let us suppose a *rocky* barrier (and that of Glen Gluoy is rocky) to be elevated, by such movements as those now in progress in South America, across the strait, separating it into two portions. Might we not expect that the high water mark would rise several feet higher, in that portion of the former channel which was still open to the sea subject to the great tidal movement, than it would in the other connected only by tortuous passages with a different sea, where the rise of the tide was small? In such a labyrinth of channels as this part of Scotland must have presented when the sea stood at the level of the upper shelves, it is even probable that there would be inequalities in the rise of the tide in different parts; I conclude therefore that when the rocky barrier was upraised between Glen Gluoy and Glen Roy, a greater tide-wave, proceeding direct from the line of the Caledonian Canal, then a great strait, swept up this deep creek; whereas a smaller one reached by a circuitous course the *Bay* of Glen Roy, which, moreover, was connected by some other straits with the eastern sea.

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* Edinburgh Transactions, vol. ix. p. 35.

Whoever walks over these mountains, and believes that each part has been successively occupied by the subsiding waters of the sea, will understand many trifling appearances, which otherwise, I believe, are unintelligible. Thus in Upper Glen Roy he will see in the level expanse, an old bay, filled up and leveled with tidal mud. Again at the Gap of Glen Collarig, with its flat bottom and cut off sides like a gateway, he will recognise a channel, at last choked up with matter drifted by the tides, and now left in the state in which it was when the waters retired from it. The traces of supernumerary shelves will offer no perplexity to him, and will equally receive with the others a simple explanation. By the theory of the sea having acted at successive levels over the whole surface of the land, the great beds of shingle* and sand, such as those near the mouth of the Spean, have a cause assigned to them adequate to the effect. Lastly, the manner in which the deposits near the mouths of the larger valleys have been modeled into successive terraces, which in some parts at least appear not to have been formed by the river, receives elucidation. I may add, that in South America I have observed numerous instances of terraces in every respect similar to these, with sea shells abundantly scattered on their surface; and therefore where there could exist no obscurity regarding their origin.

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* I have before alluded to the fewness of the well-rounded pebbles near the upper shelves, excepting at the heads of the valleys, or on flat places. This is a difficulty; though it is one common to many regions, where we know that much denudation has taken place at some period or at another. Pebbles of most rocks may in the course of time decay, but those of quartz I should think (although SCORESBY says this rock yields to the frosts of Spitzbergen) would be imperishable: if so, how comes it that quartz pebbles are not scattered over the surface of every mountain in which that rock is present, and in which the form of the land, its denuded state, or the presence of truncated dikes show that it must once, although perhaps countless ages since, have been beaten by the waves of the sea? Such pebbles, however, are not found on every mountain thus circumstanced: the explanation, I presume, rests in this; that every cause of disturbance, wind, rain, earthquakes and the fall of fragments all tend to move the pebbles in one direction alone, namely downwards. I am inclined to believe this view is

In concluding this part of my paper I will recapitulate the course of the argument pursued. 1st. It is admitted by every one that the horizontal shelves are ancient beaches. 2nd. I showed that no lake theory could be admitted on account of the overwhelming difficulties in imagining the construction and removal at *successive* periods of *several* barriers of immense size, whether placed at the mouths of the separate glens, or at more distant points. 3rd. The alternative that the beaches, if not formed by lakes, must of necessity have been so by channels of the sea, was not advanced, only because it was thought more satisfactory to prove from independent phenomena, that a sheet of water *gradually subsiding* from the height of the upper shelves to the present level of the sea, occupied for long periods not only the glens of Lochaber, but the greater number, if not all the valleys of this part of Scotland; and that this water must have been the water of the sea. 4th. It was stated (the strongest argument being the ascertained fact of the land rising at the same time in one part and sinking in another,) that in all cases the land is the chief fluctuating element; and, therefore, that the above change of level in Scotland, independently attested by marine remains at considerable heights on both the eastern and western coasts, implies the elevation of the land, and not the subsidence of the surrounding waters. 5th. It was shown that in all such prolonged upward movements it might be predicted, that there would be intervals of rest in the action of the subterranean impulses. 6th. By an hypothetical case, the land was subjected to the above conditions, and its surface was found to be modeled in a manner wholly similar, even in detail, to the structure of the valleys of Lochaber as they now exist. 7th. The true theory being considered thus established, objections to it from the non-extension of the shelves, and from the absence of organic remains at great altitudes, were answered and shown not to be valid. 8th. Many points of detail in the structure of the glens of Lochaber, were shown to be easily explicable on the supposition, that the valleys had been occupied by arms of a sea subject to tides, and which had gradually subsided during the rising of the land. Having attentively considered these several and

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correct, and that in the course of time, such pebbles are all rolled down, from having found on an isolated mountain of quartz in South America (the Sierra Ventana) a superficial patch of conglomerate, like part of an old beach, which seemed solely to owe its preservation to the pebbles having been cemented to the parent rock by oxide of iron, in the same manner as not unfrequently may be observed on some existing sea beaches. In the case of the shelves of Lochaber, it is probable, that only a few pebbles were originally formed, owing to the small power of the waves on the steep and protected shores of these ancient sounds.

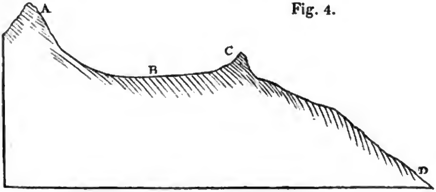
independent steps of the argument, the theory of the marine origin of the "parallel roads of Lochaber" appears to me demonstrated.

I may here remark, that MACCULLOCH seems to have been aware of the great difficulties attending his theory: but having proved that the roads could not be works of art, or the effects of any great debacle, he argued, to use his expression from the dilemma of the case, that they must have been formed on the shores of a lake. The idea of a continent slowly emerging from beneath the sea, appears, and it is a very curious point in the history of geology, never to have occurred to him as a possibility, although he was so bold and ingenious a speculator. His paper was read in the beginning of 1817, and when we reflect that during the few latter years, proofs of such movements have accumulated from all quarters of the world, we must recognise how much of this all important change (the foundation-stone, I may add, of this paper) is due to the Principles of Geology by Mr. LYELL.

Section VII.—*On the erratic boulders of Lochaber.*

I will now pass on to some other considerations which partly derive their interest as dependent on the truth of the foregoing theory. I have said, that the parent rock of many of the fragments lying on the shelves is not found in the immediate neighbourhood. These erratic boulders are generally of granite, and are from one to five and six feet in diameter; they are not confined to the shelves, but are scattered on the sides of the mountains. On the summit of the insulated hill of Meal-derry, above the level of the 972-feet shelf, there was one of large size, together with some well-rounded pebbles of rocks, which, I believe, do not occur there. In the gap of Glen Collarig the boulders on and near the upper shelves are frequent, as they likewise are in the pass between Upper and Lower Glen Roy; they occur also abundantly at the bottom of the latter valley, and on the side of Tombhran. From having found them in almost every part which I examined, I have little doubt that they are distributed in numbers over all the valleys and mountains, at least, to an elevation as great as that of the upper shelves: I make this latter restriction, because having ascended the mountains only in a few places above that level, I cannot speak positively with respect to the greater heights. On the mountains, however, between Glen Roy and Glen Gluoy on a hillock north-north-west (*magnetic*) of the summit of Ben Erin, I found several masses of granite, one of which was four feet by three in width and two in thickness (together with a couple of pebbles from rocks not *in situ*) resting on the surface of the gneiss. This hillock seemed to be entirely composed of the latter rock, and it was separated from all other hills by a valley. On the flanks of Ben Erin at about the same level, there were several boulders of granite, one of which was six feet across. Of those on the hillock (probably there were many others which I did not see in merely crossing the mountain,) the highest one was found by comparison with the Glen Gluoy shelf (by means of the barometer), to be 2200 feet above the level of the sea. I will describe in detail the spot where I found one other boulder, in as much as the whole of the district being composed of gneiss, it might be suspected that patches of granite occurred high up on the slopes of the mountains, and that the fragments had simply rolled down into their present positions. This, however, could not have happened in the case last described, nor in the following one: about twenty feet below the summit of a very sharp peak (1600 to 1700 above the sea) the whole of which consisted of tortuous layers of gneiss, there was a block of syenite with pink felspar, two feet eight inches across. The peak is wholly separated (as shown in the wood-cut, fig. 4.) from a lofty mountain also of gneiss, by a *broad* and quite flat valley, the highest part of which is 215 below the spot where the boulder lay. I may observe that I did not anywhere see another boulder of the syenite, nor a single one of granite on this side of the mountains, which is separated by a lofty ridge from the valleys of Glen Roy and Glen Gluoy, where the blocks of granite are so numerous. Between two branches, however, of the Tarf Water (which enters Loch Ness near Fort Augustus) on the summit of a hillock of gneiss, about 1200 feet above the sea, I noticed one of granite.

Fig. 4.



A. Lofty mountain of gneiss.
B. A peat moss 215 feet below the boulder, dividing the waters flowing on each side round the hill C.
C. Boulder of syenite resting on gneiss 1600 or 1700 above the sea.
D. Habercalder in the great glen of Scotland.

The granite of all the boulders which I observed in Glen Roy, and likewise of those on Ben Erin, has a uniform character; it is subject to much disintegration, and therefore I do not doubt that the boulders were originally much larger. In Macculloch's Geological Map of Scotland, the nearest granite *in situ* to the boulders on Ben Erin is seen to be at the source of the Roy, near Loch Spey, a distance in a north-east line, passing over mountain and valley, of between five and six miles. The granite there has the same lithological character with that of the boulders, and I do not doubt that it is the parent rock, at least, of those strewed along the course of the Roy. With respect to the boulders on Ben Erin, they are completely cut off from every granitic district by valleys, the *highest* point of which is 920 feet below that boulder, the altitude of which I measured; that is, it would be impossible to walk from granite *in situ* to these boulders without ascending at least that number of feet.

I will only further add, that if a sheet of water were raised to the level of the Ben Erin boulders, there would be a line of open communication* between them and the granite of Loch Spey; although I must confess I much doubt whether in that case any of the rock *in situ* at Loch Spey would remain uncovered; and if so the origin of the boulders must be more remote. The other tracts, where granite is represented in MACCULLOCH's map, are more distant, and are separated by deeper and broader valleys from the points in question. From my limited examination of the district of Lochaber I am unwilling to generalize respecting the position of the boulders, but I think that they certainly occur most frequently on the summits of little peaks, such as on Meal-derry, or on that one of which a wood-cut has been given; and perhaps likewise in the narrowest parts of the valleys; for instance at the junction between Upper and Lower Glen Roy. I observed also a greater number on the shelves than I should have anticipated, from some of those, which had originally stood higher, having rolled down. But, I repeat, I will not positively say that such is the case; although with respect to the boulders on the peaks, as I observed five well-marked cases, even during my short examination of the country, I have little or no doubt that the observation is correct.

On any conceivable theory of the transportation of erratic blocks, whether by some overwhelming debacle, or by floating ice, or any other means, it will at once be evident that they must have been scattered over the country, either before the shelves were formed, or at the time of their formation, but not on account of the delicacy of the lines at any after period. According to the generally received opinion of geologists, the so-called "erratic block period" is recent, and therefore we obtain a rude method of estimating the age of the shelves, and consequently of the elevation of the whole central part of Scotland, at least to a height of 1278 feet (or that of the upper shelf) above the sea.

It may perhaps be worth while briefly to compare together, under the conditions here afforded, the two theories of the transportation of erratic boulders, which are alone worthy of consideration, namely, that of great debacles and of floating ice. I will not lay any stress on the difficulty of imagining, in accordance with the first theory, a rush of water so impetuous as to transport vast masses of rock across profound valleys and up the steep sides of high mountains, for this difficulty has no special reference to the case of Lochaber; but those who believe in the past occurrence of so terrific an agitation of the waters of a deep sea, must in some manner account for the frequency of boulders in the most exposed places on the summits of hillocks, and likewise for so many having been left in narrow straits, where one would have anticipated the most impetuous rush of water. On the face of Tombhran I observed many boulders scattered on the shelves, which have been formed there not only by the accumulation of loose matter, but also by the deep excision of the solid underlying rock. Again, there were other boulders on the shelves on the rocky peninsula near the junction of Upper and Lower Glen Roy, where much of the gneiss has been worn away. Here it was not possible, from the non-existence of higher land, that the boulders could have rolled into their present places from above, after the formation of the shelves; nor was this at all probable in several parts of Tombhran. On the supposition of the boulders having been originally scattered over the country, and the shelves formed at a subsequent period, we have the difficulty, though perhaps not an insuperable one, as we do not know their original size, of believing that blocks of granite have been preserved for a long period on those very places, where a zone of gneiss had been cut into and worn away. Some of the boulders on Tombhran were lying on the surface of the lower edge of the shelves, in parts where, as above said, I fully believe the inclination of the ground was so trifling that it was impossible they could have rolled down from above; but I regret much that I omitted, from not having perceived its importance, to ascertain this point with certainty. If the fact be so, and I scarcely doubt it, it would prove that some action, so quiet as not to have disturbed the small quantity of earth and little stones, of which the shelves are formed, transported these boulders across deep arms of the sea, and left them on the surface of the ancient beaches. The theory, that all erratic blocks, circumstanced like these of Lochaber, have been transported by floating ice, wholly removes these difficulties; for the icebergs, in the first place, would generally land the fragments, with which they were charged, on the lower part of the beaches or shelves; and secondly, those which had arrived not long before a fresh elevation would have been exposed only to a small amount of tidal degradation. Thirdly, the icebergs would frequently be stranded on shoals and islets, over and round which the tides swept; and likewise they would be frequently driven on shore in the narrow parts of the channels, where the waters were pent up. So that in after times, when the land was drained, it is easy to perceive that the boulders would lie scattered in such places, as they now actually occupy in the district of Lochaber. Lastly, this theory requires that every district where boulders are found should have been covered by the sea; here we have independent proofs that such was the case, at least to an elevation of 1278 feet.

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* This is a similar fact to what has been observed on the Jura. Sir JAMES HALL (Edinburgh Royal Transactions, vol. vii. p. 143.) says "it is principally where the snowy summits are visible from the face of the Jura by means of some depression in the intervening hills, that we find these travelled masses."

In my Journal during the voyage of the Beagle, I have endeavoured to show that the erratic blocks of central Europe were probably transported at that period*, when its climate was more equable (chiefly consequent on the larger area of water), which favours a low limit of the snow line, and therefore the probability of glaciers, the parents of icebergs, descending in favourable places into the sea. It is therefore to this period, if this view be correct, that we must refer the "parallel roads of Lochaber," and consequently the elevation of the land, not only of the 1278-feet portion (which it is certain has been elevated at an epoch not distant), but likewise of the whole altitude, whatever it may be, at which boulders occur. If there be others, as is most probable, at a greater height than that one on Ben Erin, which I observed in merely crossing the mountains at a point 2200 feet above the sea, then by so much the greater has the elevation of the land been within this same period. Mr. Blackadder (in a letter to Mr. Lyell) states he has seen on the west coast of Scotland, in the island of Mull, large fragments of quartz rock at the height of 2000 feet, of the same description as that found on some of the adjoining islands and mainland. In Sweden M. Sefström says that boulders occur at an elevation of 1500 feet; in Massachussets, in North America, they are found, according to Professor Hitchcock, at 3000; and on the Jura it is well known they occur, from low down, to an altitude of 4000 feet. It is interesting to discover, that in our own country the upward movements, within *the same period*, have been more than half as great as those which have affected the latter colossal chain. But regarding the exact period, allowance must be made, since on the one hand the glaciers of the Alps, situated ten degrees nearer the equator than those on the mountains of Lochaber, must have much earlier retreated upwards, and failed in descending to the level of the sea, during the change from the former to the present climate; whilst, on the other hand, to counteract the equatorial influence, they were appendages on a greater mass of snow accumulated on far loftier chains.

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* I refer, of course, only to the more temperate and central parts of Europe, but it appears that boulders are sometimes transported in these regions, even at the present time. Sir James Hall, in his Memoir on the "Revolutions which have affected the surface of the earth" (Edinburgh Transactions of the Royal Society, vol. vii. p. 157.), states that in the Solway Firth (and therefore in salt water) "a large block of stone, four or five feet in diameter, lying within high-water mark, and well known as having served as the boundary of two estates, was during a stormy night in winter transported ninety yards, and the persons on the spot were convinced that this migration was performed by means of a large cake of ice, formed round the stone, and attached to it, and that the whole had been lifted and carried forward by the rising tide. The course of this stone was

Section VIII.—*On the small amount of alluvial action since the formation of the shelves.*

I now pass on to another consideration. Macculloch was much struck with the fact, that in many cases where a shelf crossed a rivulet, I mean one of those silver-like threads of water which descend the flanks of steep mountains in nearly straight

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marked upon the sand below by a deep and broad furrow, which remained visible for a long time afterwards, as I have been informed by several members of the Society, who saw it after an interval of more than a year." I presume from the position of the stone as a land-mark, and from the distance it was transported by the *rising* tide, that the furrow left by its passage must have been either oblique or parallel to the shore. What would have been the effect if this large and heavy block had been pushed over a surface of solid rock instead of sand? This question will recall to the mind of those who have read the late papers of Messrs. Charpentier, Venetz, and Agassiz, the case of the longitudinally and obliquely scratched rocks of the Alps. In the Addenda to my Journal during the voyage of the Beagle, I have endeavoured to show that the passage of ice, with imbedded fragments of rock, acting at successive levels on the surface of shoals during the gradual rising of the land, offers the most probable explanation of the scratches and grooves, which have justly excited so much attention in Scotland and other places.

lines, it frequently entered a little way on each side of the gully. From this fact it is evident that the gully must have been partly formed before or at the time when the shelves were sea-beaches. I particularly observed several instances of this structure. One which struck me most was in Glen Roy, opposite a gap in the mountain which leads to Glen Fintec; here two small threads of water were united at the point where the line of shelf crossed them, and at their junction the rock was much exposed, so that any one would have supposed that the furrow in which they flowed had been entirely hollowed out by their action. But the shelf curved in a little way on each side; and, what was more curious, the apex of turf above the point of junction of the two streamlets had evidently originally formed part of the shelf. By this it was shown that the entire hollow, with the exception of the actual beds of the streams, must have existed as an indentation or little cove on the line of ancient sea-beach. It appeared to me that the extent to which the shelves entered these furrows did not bear any close relation to the power of the streamlets now flowing in them: thus on Tombhran (in front of the houses of Roy) a great gorge which is impassable, and where the rock is bare and shattered, has been deeply cut into by the winter torrents, and yet the shelves enter only a very little way on each side; whereas in other cases we find a hollow or creek of some size, but with an insignificant stream flowing in it, for instance, that opposite the gap of Glen Fintec, which has not even removed the remnants of the shelf from the head of the gully, in which it has flowed ever since the retreat of the sea.

Without entering here into a full consideration how these gullies were originally formed, and whether the indentations made in the beach at one level might not be produced downwards to another, I will only remark, that the sea in most situations certainly does alter the form of its coast, and yet that an accurate map of any shore gives a line indented in such manner, that a series of them, if placed one above and a little behind another, would produce the same kind of furrowed surface which characterizes the mountains of Lochaber, as well as most others. I will further observe, that when travelling along the shores of northern Chile and Peru, where the alluvial action is reduced to an exceedingly small measure, and where it is not probable that within a recent period there has been any *great* change of climate, I was repeatedly much surprised at observing how absolutely similar all the minor inequalities of the surface (yet covered with beds of sea shells of existing species) were to those of countries, where almost every detail in outline is usually attributed to meteoric agency; I could perceive only one difference, namely, that the larger valleys had unusually flat bottoms. Although fully convinced of the truth of this fact, I confess I was astonished at discovering in the mountains of Scotland, which have been exposed during a vast period to the destroying action of a wet and boisterous climate, clear proofs that almost every furrow and inequality has been left nearly in the state in which we now see it*, by the retiring waves of the sea. From the preservation of some of these beaches, one can point to the very spot, and declare so much was removed when the sea stood there, and so much since by the running streams of fresh water.

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* It is scarcely possible to convey by language any accurate idea of the kind of inequalities which, from the

It may be asked, has the present alluvial action done nothing here? Something it assuredly has done, but I repeat, comparatively nothing to that which was effected before the sea retreated. In Chile I concluded that the action of the more rapid rivers and torrents was chiefly confined to removing the littoral and sublittoral deposits left by the arms of the sea; and secondarily in cutting, as soon as the upper beds were removed, a wall-sided gorge through the solid rock. It appeared, that as long as the river had its passage through the water-worn materials, from the great facility with which it changed its course, its bed was broad, but as soon as it reached the solid strata it became exceedingly narrow. These conclusions are in strict conformity with what I observed in the glens of Lochaber. Of the small amount of corrosion effected since the sea stood at a level of the upper shelves, there are some curious instances. Sir LAUDER DICK, in describing in detail the head of Glen Gluoy*, concludes that the river has worn there, during the immense period which must have elapsed since the water (of the sea) retired from the 1278-feet shelf, a remarkable chasm, between fifty and sixty feet in depth, but only a *few feet* wide. The stream in the northern arm of Glen Turet has cut for itself a passage in the solid rock in only a part of the valley, between the middle and the 972-feet shelf. In Upper Glen Roy the southern stream falls into the plain by a cascade, to the upper edge of which on each side the 1226 shelf approaches close. I did not ascend the spot, but as far as I could judge, the water has not cut back more than at most a few yards, into the rock over which it falls. Other similar instances might be adduced. Although none of these streams form great bodies of water, yet when flooded by the winter rains they cannot be inconsiderable; and their action has been prolonged for so vast a period, that the geographical features, together probably with the climate of the country,

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shelves passing over them and into the intervening hollows, we know were so left by the sea. I hope any one who feels interested on this subject, will carefully examine the plates accompanying Sir LAUDER DICK's paper (Edinburgh Transactions, vol. ix.), and especially Plate IV. The shelves on the left side (looking up the glen) bend into all the principal gullies; and on the right side, directly in front of the foreground, by looking close at the plate they will be seen to curve a little way into each of the perpendicular furrows (some thinning out and others commencing), the bottoms of which have evidently been much deepened by the descending streamlets. The idea given by these plates of the state of surface in these mountains, and of the manner in which the shelves bend round the headlands and enter the gullies, appears to me exceedingly faithful; although the glen itself, as represented, is too narrow and profound, and the sides much too steep. To view this Plate is a lesson full of instruction to the geologist, for he will scarcely fail to be astonished when he sees that the drawing is characteristic of any ordinary valley in a mountainous country, and at the same time to find himself compelled to admit, that even the little furrows, which it might be thought had been formed but yesterday, must have owed their origin, at least in great part, to the successive coves or indentations, continued one below another on ancient sea-beaches.

* Edinburgh Transactions, vol. ix. p. 26.

have been greatly changed. The rocky crests of the mountains no doubt have suffered from the weather; but the perfection of the shelves over spaces many hundred yards in length, and in the case of Glen Roy (where the three shelves occur) of some hundred feet in vertical height, clearly proves that as the sea left the greater part of the surface, so does it now remain. Amongst mountains the bursting of temporary lakes may sweep away or accumulate vast quantities of rubbish in the valleys; earthquakes may hurl down piles of fragments; and torrents during the lapse of ages, or under favourable conditions (such as the descent of many pebbles), may excavate a gorge of almost any depth, but which, as far as it is possible to judge, will always be narrow and steep-sided. All this must often have happened, and will so again; but the glens of Lochaber plainly show that the effects of ordinary alluvial action is exceedingly small, far smaller than any one would have anticipated. And as their outline does not differ in any marked degree from that of all other valleys, this conclusion may be extended to other cases.

In Glen Roy, where the three shelves can be seen near each other, little or no difference can be perceived in their state of preservation; indeed the upper one, I think, is more perfect than the one below it. From this fact an argument has been advanced by Dr. Macculloch, that no long interval of time could have elapsed between their formation. But this view is quite inadmissible; either the worn and deeply notched rock of the shelves on Tombhran, or the buttresses on the middle shelf (as at the head of Lower Glen Roy), which are composed of large masses of well-rounded shingle, is sufficient, without considering the *intermediate shelf* and other appearances, to *prove* that the water must have remained at levels intermediate between the highest and the 972-feet shelf for very long periods. Hence the alternative is obvious, and is in direct accordance with what has already been advanced, namely, that the ordinary alluvial action is so exceedingly small, that whether the surface has been exposed during one, two or more whole epochs, no sensible difference can be perceived in the state of its conservation.

Of the many remarkable features in the geology of this district, few, perhaps, are more remarkable than this perfect preservation of its surface. We have a mound composed of soft materials so small, that it oftentimes cannot be distinguished, by a person standing on it, from the adjoining slope, but which it is not probable, from the structure of the mountains, was ever much larger; and yet this very mound, when viewed from a distance, will be seen to extend for many hundred yards, even miles, continuous and perfect, with the exception, perhaps, of a few small breaks, where some streamlet descends. On these same mounds we can sometimes distinguish those fragments which have been washed by the little waves of the ancient waters, from others which have since fallen; and at Loch Treig, at the height of 972 feet above the sea, the tide-scooped rocks appear as if scarcely a century had elapsed since they were washed by the ripple of the eddying currents. The preservation of the druidical mounds in Britain has often been adduced as a circumstance worthy of attention;

but here during a period which cannot be reckoned by thousands of years, but only by those great revolutions of nature which are the effects of slow and scarcely sensible changes, works smaller than those ancient ones dedicated to superstition, retain each outline nearly as perfect as when first formed by the hand of nature.

These facts are interesting under another point of view, for they prove to us that we may trust the plain inference of our experiences. Although we see* the stone of many ancient buildings decaying and crumbling away, yet we know that others, as the obelisks of Egypt, have lasted more than three thousand years, with the hieroglyphics nearly perfect on them: now we cannot see any reason why their general outline, even in points of detail, should not last a hundred times three thousand years. Again, although we might expect the crest of a mountain range to be shattered, and the bed of a torrent to be worn down more or less deeply, yet if we look at a convex slope of soil clothed with turf, and drained on each side by rivulets, we can see no reason, as long as the vegetation is persistent, why such a slope (with the exception of any spot where a waterspout might burst, or a stroke of lightning fall) should not last for as many thousand centuries as the obelisks of Egypt shall remain entire. Of the justice of these inferences, conclusive evidence is afforded by the state in which we now see the mountains of Lochaber,—a state of which we approximately know the high antiquity.

Section IX.—*On the horizontality of the shelves, and on the equable action of the elevatory forces.*

Sir Lauder Dick, with Mr. Maclean's assistance, seems to have determined within very small limits the absolute horizontality of the several shelves. A delicate eighteen-inch levelling instrument, made by Jones, was employed. Sir Lauder says†, "Directing the object-glass of the instrument to the nearer, and immediately opposite corresponding line of shelf, it applied all along most accurately to the horizontal hair; but when pointed to those further off (some of which were perhaps five or six miles distant), they appeared to sink sensibly below the hair, and this in proportion to their distance from the point where we stood; but they were nowhere observed to do so in a greater ratio than the allowance for the curvature of the earth at such rectilineal distances demanded. And, what was in our opinion most conclusive, when the telescope was pointed to, and made to traverse along any particular portion, which, from being directly opposite to the eye, might have been presumed to be nearly equidistant in all its parts, it was found to preserve an uniform relation to the horizontal hair." The same results were obtained in other instances; but yet the angle of depression of the distant shelves does not appear to have been actually measured, and its correspondence with the curve of the earth calculated. But it is quite certain that if any difference from that curve exists, it must be very small*. Here then is a case which supports apparently with more weight than perhaps any one hitherto advanced, the doctrine that the land is the stationary element in these changes of level, and the ocean the fluctuating one; for it may well be asked, can we suppose that a whole country shall have been lifted up without the smallest ascertained flexure of the ancient coast lines? Without reverting to the argument of the movements now in progress, some upwards and some downwards, or to the difficulty of imagining a receptacle for a stratum of water, nearly 1300 feet thick, concentric with the globe, I will consider the phenomenon in another point of view. It appears from the facts given by Mr. Lyell in his Principles of Geology†, and in the Philosophical Transactions‡, that a large territory in Sweden is now rising at the rate of three feet in a century; and that the area affected reaches from Gottenburgh to Torneo, and thence to North Cape (a distance of 1000 geographical miles), although the rate of elevation increases as we proceed northward. We may therefore safely conclude, that large spaces in Scandinavia have been elevated so equably, that at points several miles, if not leagues apart, the difference of elevation at the close of the past century, did not amount to one foot. In South America the whole coast of Chile has been elevated within the recent period; and during the great convulsions which affect that country, large spaces have been uplifted nearly to the same amount, although some parts a few feet more than others. On the eastern side of the same continent, the land has also risen within the same period, and as earthquakes are unknown there, the change probably has been, as in Sweden, so slow as to be insensible at any one time. On that side the traveller may ride for many hundred miles over plains, scarcely broken by a single undulation, and where the strata and surface are almost absolutely level: no one would there for one moment imagine that the elevatory forces had acted unequally, but rather he is astonished that the bottom of any sea or estuary should have been so uniform, as must have been that of which the plains of La Plata not long since formed the bed.

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* Consult Professor Phillips's interesting paper on this subject. Geological Proceedings, vol. i. p. 323. April, 1831.

† Edinburgh Transactions, vol. ix. p. 8.

If then great plains and mountainous countries can be raised within such small limits of absolute horizontality, as undoubtedly has happened in the above cases, shall we, who are wholly ignorant of the mechanism of these movements, be justified in rejecting the plainest analogies, in supposing difficulties little short of physical impossibilities, and in believing that the reverse of what is ascertained in other cases has taken place in Lochaber, and all simply because the change of level has been more equable, than we in our ignorance could have anticipated? Every one, I think, who will attentively consider the above facts, will answer with me in the negative, marvellous though the fact be, that the beaches of Lochaber, raised on high so many hundred feet, should still follow the curvature of their ancient waters. On the contrary, a most important geological fact is established; namely that an area (twenty miles in length and eighteen broad, and perhaps more, if the shelves on the banks of the Spey be included in it) has been raised 1278 feet above the level of the sea, so equably, that no deviation from the true curvature of the earth can be discovered by the ordinary means of leveling*.

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* I may here remark, that the equal elevation of the west coast of Scotland, and indeed of the whole British Islands and other parts of Europe, may be inferred from the facts collected by Mr. Smith in his paper in the Edinburgh New Philosophical Journal. This author says (vol. xxv. p. 388.), "The great terrace (known to be of marine origin from the presence of organic remains), the base of which seems *very generally* to be between *thirty and forty feet* above the sea, forms a marked feature in the scenery of the west of Scotland."

† Book II. chap. xvii. On the gradual rise of the land in Sweden.

‡ Transactions of the Royal Society, Part I. 1835, p. 33.

Section X.—*Speculations on the action of the elevatory forces, and conclusion.*

If we choose to enter on speculative grounds and to reflect on the secondary means which have caused these equable movements, two solutions occur. But first I must remark that the crust of the earth seems to yield easily to the forces which have acted on it from below; when we observe a brick-wall dashed to pieces by a cannon ball, or a pane of glass by a small stone, we say that both are fragile and yield easily; so when we examine the earth and find it fissured and refissured, one fragment let down and another raised high up (as we know to be the case where extensive sections have been obtained, as in our coal-pits or metalliferous districts), we must certainly admit, that the force which has broken up the crust in vertical planes relatively nearer to each other, compared with its thickness, than in the fissured pane of glass, easily overcame the resistance offered to it, however absolutely great that may have been. This same conclusion is forced on us, when we reflect that the very cause of the trembling of the ground in earthquakes seems due to the rending of the strata; and that earthquakes in many countries are of such frequent occurrence, that probably this hour will scarcely elapse without the crust somewhere yielding. If indeed the crust did not yield readily, partial elevations could not be so gradual as they are known to be, but they would assume the character of explosions. That there has been some real connexion in certain cases† between that state of the weather which is accompanied by a low barometer and the occurrence of earthquakes, can, I think, hardly be doubted; if we admit Mr. P. Scrope's explanation of this, that the diminution of atmospheric pressure (equal in some cases to an inch and half of mercury, spread over a very large area) determines the particular time at which the earthquake occurs, the force and tension being before almost balanced, we may be said to possess a rude measure of the force requisite in that area to overcome the coherence of the parts, as existing in the intervals of the recurrent earthquakes. If then the motive force acts so gradually that the earth's crust can acquire that degree of tension, which causes large portions of it to yield readily to a very slight additional impulse; and if, as we know undoubtedly to be the case, the crust has yielded in innumerable vertical planes, intersecting each other like a net-work, and running parallel to each other at very short distances, we are compelled to admit that the equable elevation of so large an extent of country as Lochaber, must have resulted from the equable action of the elevatory forces, and not from the cohesion of its parts.

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* Considering the great importance of this conclusion, and the many points of interest connected with the subject of the 'parallel roads,' it is greatly to be desired that the admirable opportunity for a close examination, afforded by the intended Ordnance Survey, will be taken advantage of by the gentlemen, so well qualified for the task, who conduct it.

† In my Journal during the voyage of the Beagle, I have mentioned (p. 431 and 432.) some instances of this.

Bearing this in mind, the most obvious solution, but I very much doubt whether the correct one, is, that no force excepting the uniform expansion of solid matter from heat, could raise so equably the surface of a great *fragile* mass, as the district of Lochaber must be considered. I doubt this solution, first, because a very great expansion is necessary, especially if we include in these movements the elevation of the erratic blocks, now lying more than 2200 feet above the sea. Secondly, because the movements appear to have been of the same kind as those in the not distant country of Sweden; and there it has been shown by Mr. Lyell, that near Stockholm an alternate movement of more than sixty feet has taken place within the human period; and one is strongly tempted to believe that there is some relative connexion between the areas in Northern Europe which are rising and those which are quietly subsiding. These facts to be explicable on the theory of expansion, require, as it appears to me, far too capricious an action, in so *slowly* and far-pervading an influence as heat, to be admitted; whilst on the supposition of mechanical displacement such difficulties are not presented. Thirdly, because (and it is my chief reason for rejecting the agency of expansion by itself) the movements appear to have been of the same order with those now in progress in South America; and in that country the elevation of certain wide areas, as I endeavoured to show in a paper lately (March 7, 1838) read before the Geological Society, cannot be attributed to any other cause than an actual *movement* in the subterranean expanse of molten rock: to speak only for example sake, such as would result from a change in position of those inequalities in the ellipticity of the earth's surface, which seem indicated by the measurements of arcs of meridians. It may also be inferred, from the facts given in that paper, that the fluidity of the nucleus must be tolerably perfect. In the volcano, even the lava which is propelled to the summit of a mountain, far beyond the subterranean isothermal line of melted rock, and poured out on the surface, is oftentimes so fluid, that it runs into thin sheets like molten metal. Also at the junction of the plutonic with the metamorphic formations, we see tortuous thread-like veins branching from the former into the latter, which could only have been injected when quite liquid. Here the rock has been melted at a great depth under an enormous pressure, and yet the fluidity must have been very perfect: such plutonic rocks moreover form the beds on which all others rest. Considering these latter facts, together with the inferences deduced from the phenomena observed in South America, it may be granted as not improbable in any high degree, that this part of Scotland when it was upraised rested on matter possessed of considerable fluidity, which underwent a slow change of form. If this be granted, there is no great difficulty in conceiving that the surface of the interior molten matter might retain that degree of curvature proper to it, as the resultant of the unknown force with that of gravity and the centrifugal impulse. Moreover, as we must conclude from what we now see going on in South America and in Scandinavia, that the area affected was large, the difference between the amount of curvature of the fluid nucleus after the rise in that part of one or two thousand feet, would be exceedingly small, and its outline scarcely distinguishable from that of the ocean, and certainly not from that of a sea affected by various tides in confined channels, which in the case of Glen Roy affords the only standard of comparison. We may almost venture to say, that as the packed ice on the Polar Sea, with its hummocks and wide floes, rises over the tidal wave, so did the earth's crust with its mountains and plains rise on the convex surfaces of molten rock, under the influence of the great secular changes then in progress.

After these considerations I am far from thinking it an overwhelming difficulty, that the curvature of the shelves of Glen Roy over a space of four, or five, or perhaps even twenty miles should appear to be the same with that of the surface of the ocean, within that limit of accuracy which the nature of the case renders possible. On the contrary, I deduce from their curvature, first, that the district of Lochaber formed only a small part of the area affected; secondly, a confirmation of the view, which I deduced from the phenomena observed in South America, that the motive power in such cases is a slight additional convexity slowly added to the fluid nucleus; and thirdly, this additional fact, that we thus obtain some measure of the degree of homogeneous fluidity of the subterranean matter beneath a large area, namely, that its particles, when acted on by a disturbing force, arrange themselves in obedience to the law of gravity. And although we arrive at this conclusion with some surprise, when relating to the abysses of the nether regions, we see it habitually verified in volcanic countries, where a torrent of lava, checked by some obstacle, has expanded into a level sheet.

Mr. LYELL, in his Principles of Geology[*], quotes a passage from Sir JOHN HERSCHEL's Astronomy[†], to show that whatever may have been the original figure of the earth, the wearing down of the solid matter and its redeposition at the bottom of the sea, must tend continually to change the *actual* figure of the earth, as PLAYFAIR[‡] expresses it, into the *statical* one: he then adds, "that the same remark applies to every stream of lava flowing on the surface, and if the volcanic action should extend to great depths, so as to melt one after another different parts of the earth, the whole interior might at length be remodeled under the influence of similar changes, due to causes which may all be operating at this moment." Now if it be granted that the curvature of the shelves of Lochaber is due to the elevation of the district by means of a subterraneous expanse of fluid matter, the atoms of which obeyed the law of gravity, it cannot be doubted they would likewise obey that of the centrifugal force. Therefore, if the figure of the earth did not already very nearly approach to that of a spheroid of equilibrium, regions near the equator and others near the poles, during the changes of level now actually in progress, would be acted on by forces greatly different; and consequently as the crust does now yield (and has yielded in an infinite number of planes,) the statical form would be immediately acquired. This view is here given, because a directly opposite, and as I cannot but think incorrect one, has been advanced by Playfair*.

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[*] Principles of Geology, Book II. chap. xviii. p. 311. 5th edit.

[†] Cabinet Cyclopedia. Astronomy, p. 120.

[‡] Illustrations of Huttonian Theory.

In concluding this paper, I will briefly indicate the chief points which receive illustration from the examination of the district of Lochaber by Sir Thomas Lauder Dick, Dr. Macculloch, and myself. 1st. Nearly the whole of the waterworn materials in the valleys of this part of Scotland were left, as they now occur, by the slowly retiring waters of the sea; and the chief action of the rivers since that period has been to remove such deposits; and when this was effected, to excavate a wall-sided gorge in the solid rock. 2nd. During the vast period which must have elapsed since the sea stood at the level of the upper shelves, the alluvial action has been exceedingly small: steep slopes of turf over large spaces and the bare surface of rocks have been preserved even perfectly; and we see every main, as well as most of the lesser inequalities of the land, in the state in which they were then left. 3rd. The elevation of this part of Scotland from the level of the present beach to the height of *at least* 1278 feet has been extremely gradual, and was interrupted by long intervals of rest: it has taken place since the so-called "erratic block period." 4th. It is probable that the erratic blocks were transported during the quiet formation of the shelves. One was observed to occur at an altitude of 2200 feet above the level of the sea. 5th. The extraordinary fact that a large country has been elevated to a great height so equably, that the ancient beach-lines retain the same, or nearly the same curvature, which they had when bounding the convex surface of the ancient waters. Lastly. The inferences from this head, supported by other cases, namely, that a large area must have been upraised, and that this was effected by a slight change in the convex form of the fluid matter on which the crust rests; and, therefore, that the fluidity is sufficiently perfect to allow of the atoms moving in obedience to the law of gravity, and consequently of the effects of that law modified by the centrifugal impulse. Hence, that even the disturbing forces do not tend to give to the earth a figure widely different from that of a spheroid in equilibrium.

Postscript.

I am much indebted to my friend Mr. Albert Way for his kindness in lending me the drawing, from which the accompanying lithographic sketch has been taken. It very faithfully represents the general appearance of Glen Roy.

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* Illustrations of the Huttonian Theory, p. 488.

V. *An Account of the Fall of a Meteoric Stone in the Cold Bokkeveld, Cape of Good Hope. By* Thomas Maclear, *Esq. F.R.S. &c., in a Letter to Sir* John F. W. Herschel, *Bart. V.P.R.S. &c. &c. Communicated by Sir* J. F. W. Herschel.

Received March 7,—Read March 21, 1839.

Royal Observatory, Cape of Good Hope,
November 24, 1838.

Dear Sir John,

A METEOR exploded on the 13th of October in the Cold Bokkeveld, with a noise so loud as to be heard over an area of more than seventy miles in diameter, in broad daylight, about half-past nine in the morning. It was seen traversing the atmosphere north-east of the point where it exploded sixty miles, of a silvery hue, the air at the time calm, hot, and sultry. The barometer chanced to be observed at Worcester, where the air was also calm and hot. It stood at the lowest point of its range, but, from the construction of the instrument, that point cannot be noted in inches unless by comparison with another, which I will endeavour to have done the first opportunity.

The explosion was accompanied by a noise like that from artillery, followed by the fall of pieces of matter, of which I send you the largest and best specimen I have seen, procured by Mr. Watermeyer. Portions fell or were dispersed on the ground at the distance of an hour, or five miles from each other. Some falling on hard ground were smashed; others on moist ground plunged into the earth; and I am told that one piece made a hole as broad as three feet, and sunk deep. It is stated to have been so soft as to admit of being cut with a knife where it first fell; then it hardened, but I cannot learn anything as to its temperature at that moment. If the reports are correct, I estimate the original solid mass at five cubic feet, viz. the sum of all the portions that fell to the ground.

That which I send to you is a good specimen, for the fracture is exactly similar to those I have seen that fell elsewhere, but, from being broken into small pieces, few of them have any crust or outside to show the state of fusion. This exhibits that state all over: when the two pieces are applied to each other they exactly fit, and show that it was in a state of ignition when it separated from the rest in the air.

Mr. Judge Menzies told me he was returning from circuit accompanied by Mr. George Thompson. On the morning of the 13th "he was in the bush," about sixty miles from the Bokkeveld, on his way homewards. The air was hot and calm, as preceding a thunder storm, but the clouds were not dark; on the contrary, they had an unusual reddish tint. About half-past nine his attention was roused by something like a meteor, of a silvery colour, passing through the atmosphere, to which he directed the attention of those about him. The object moved in the direction of the Bokkeveld. He proceeded on his journey, and arrived in the evening at the place of Mr. De Toit, where he was told that a meteor had exploded in the morning, with a report as loud as "from three pieces of cannon," and that some of it fell close to the place, one nearly striking a person in a field.

Mr. Truter, Civil Commissioner of Worcester, was sitting in his office. He told me that the windows suddenly shook; immediately a rumbling noise followed, which he supposed was the precursor of an earthquake; his barometer stood at the lowest point of its range. Mrs. Truter heard a similar noise in the dwelling-house; other persons in the town were startled by the like noise; the next day he heard of the meteor in the Bokkeveld. The statement made to him by several persons is so like the statement in the inclosed letter of Mr. Watermeyer's correspondent, that it is unnecessary to repeat it.

Understanding that Mr. Watermeyer had obtained a portion of the meteorolite of considerable dimensions, I wrote to him to request a piece for you. He returned the inclosed reply, together with the whole specimen, wherein you will find that he had designed it for you. The clergyman's communication is clear and comprehensive.

On reference to the Observatory Meteorological Journal, there is nothing remarkable noted.

	h	Barom.	Out. Ther.	Wet.	Wind.	
Oct. 12.	9½	30·191	60·1	58·5	4 S.	8 blue.
	20⅔	·247	68·2	62·5	38½	6 b. cirri. Direction horiz.
13.	Noon	·242	74·2	67	3 SSW.	5 blue cirri.
	3⅓	·230	74·2	68·7	4 SSW.	6 blue cirri.

Therefore the effect did not extend so far.

You will find the Cold Bokkeveld on the map by carrying your finger along the parallel of St. Helena Bay.

Believe me, dear Sir John,

Your faithful Servant,

Thomas Maclear.

Translated Extract from a Letter of the Rev. Mr. FAHN *to Mr.* WATERMEYER, *dated Tulhagh,* 6 *Novem.* 1838.

The object of these lines is to fulfil my promise in sending to you herewith one of the stones which fell simultaneously during the atmospheric tremor in the Cold Bokkeveld, on the 13th of October. This stone was found between the estates of JACOBUS JOOSTEN and PIETER DE TOIT. Several have fallen on the place of RUDOLPH VAN HEERDEN, where one fell on the hard road, and was dashed to pieces. Another on a ploughed field sunk a few inches into the ground, and a third falling on a moist place near water, lodged itself to the depth of several feet. Some people say they observed smoke whilst these stones fell; and also that when they were picked up a smell was observable, as between sulphur and gunpowder.

The stone which you receive lay one hour distance from the place where the others were found. In the *same* direction in which the agitation was perceptible, viz. from N.W. to S.E. more stones were found. Some people saw in the same direction also a dark blue streak, which lost itself in a south-easterly direction. I have another somewhat larger stone in the Bokkeveld, which was too heavy for me to carry on horseback. If the latter one can be of service to you, I shall not fail to send it. This stone was found in *two* pieces, as it is at present.

Mr. WATERMEYER'S *Letter to* THOMAS MACLEAR, *Esq.*

Wednesday Morning,
21 Novem. 1838.

MY DEAR SIR,

I have to thank you for the favour of your note of Saturday last by Doctor KRAUSS.

As soon as I received the accompanying specimen, it was destined by me for our much-esteemed friend Sir JOHN HERSCHEL. You will therefore perhaps have the kindness to transmit it to Sir JOHN, with my sincerest regards, by some fit opportunity. I have added (in the preceding extract) whatever little information Mr. FAHN'S letter contains on the subject. I shall write to Mr. FAHN by this week's post, to send me the second specimen also, of which he speaks. If there be no *immediate* opportunity of forwarding it to England, it might perhaps be proper to exhibit it first at our next Institution meeting, as I have not yet had an opportunity of showing the stone to any of its members.

Believe me, my dear Sir,
Sincerely and respectfully yours,
J. WATERMEYER.

Chemical Account of the Cold Bokkeveld Meteoric Stone. By MICHAEL FARADAY, *Esq. D.C.L. F.R.S. &c., in a Letter to Sir* J. F. W. HERSCHEL, *Bart. V.P.R.S. &c. &c. Communicated by Sir* J. F. W. HERSCHEL, *Bart.*

Received March 7,—Read March 21, 1839.

Royal Institution,
February 28, 1839.

MY DEAR SIR JOHN,

I am at last able to send you a chemical account of the meteoric stone, leaving its physical characters (except some of those which bear upon the chemical results) entirely for your observation.

The stone is soft, porous, and hygrometric. A piece of it which, at common temperature, weighed 194·4 grains, by being perfectly saturated with water under the air-pump receiver, became 202 grains, and when thoroughly dried became 182·9 grains. In its most moist condition it had a specific gravity of 2·48, which, if abstraction be made of the water in it, would give a specific gravity of 2·94 for the dry stony matter.

It has a very small degree of magnetic power, and that is irregularly dispersed in the stone.

The heat of the mouth blowpipe sends off sulphur, and softens, but does not fuse it; a higher heat, after softening it still more, makes it run into a very fluid state, the globule when cold being black and opake.

The composition of the stone may be gathered from the following analytical results, calculated for 100 parts of the stone in its natural state:

Water	6·50
Sulphur	4·24
Silica	28·90
Protoxide of iron	33·22
Magnesia	19·20
Alumina	5·22
Lime	1·64
Oxide of nickel	0·82
Oxide of chromium	0·70
Cobalt, a trace	
Soda, a trace	
	100·44

I have entered the *iron* above as protoxide, and nearly the whole of it is in that state. But there are portions, though very small, of metallic iron present. I could not collect more than 0·06 from 100 parts of the stone. Of this the largest portion was in very fine particles, recognisable only by their magnetic properties, and the evolution of hydrogen by dilute sulphuric acid; but there was one piece of sufficient size to show the malleability, lustre, and other general properties of the metal. This metallic iron contained nickel as well as the stone generally. A part of the iron in the stone was also in the state of sulphuret, as was evident by the sulphuretted hydrogen evolved on the action of acids.

The result with regard to the *sulphur* was obtained in the form of sulphate of baryta; but though I have entered it as sulphur only in the analysis, it did not all have that state in the stone, for a part of it was there as sulphuric acid. In fact, water only, when boiled with the stone, removed small portions both of sulphate of lime and sulphate of soda; and this was the case when, on repeating the experiments, I was very careful to take parts from *the middle* of the smaller fragment, parts which had not seen the light until I broke them out. It is a question, however, whether the soda belonged to the stone when it fell, and what proportion of sulphuric acid was in it at that time; for the stone being porous and hygrometric, the water and air in it may have converted a part of the sulphur into sulphuric acid; and as to the soda, I think it must have been acquired upon the earth; for the water separated also a portion of destructible organic substance, and the larger fragment of the stone still has small particles of insoluble vegetable matter adhering to it, having the appearance of being derived from manure.

I am, my dear Sir JOHN,

Yours most faithfully,

M. FARADAY.

Sir JOHN F. W. HERSCHEL, *Bart.*
&c. &c. &c.

VI. *Fifth Letter on Voltaic Combinations, with some Account of the Effects of a large Constant Battery. Addressed to* MICHAEL FARADAY, *Esq. D.C.L. F.R.S., Fullerian Prof. Chem. Royal Institution, &c. &c. &c. By* J. FREDERIC DANIELL, *Esq. F.R.S., Prof. Chem. in King's College, London.*

Received April 11,—Read May 30, 1839.

MY DEAR FARADAY,

IN my last letter to you, which the Royal Society have done me the honour to publish in the Philosophical Transactions for 1838, I observed, that "the principal circumstance which might be supposed to limit the power of an active point within a conducting sphere, in any given electrolyte, is the resistance of that electrolyte, which increases in a certain ratio to its depth or thickness." The superficial measure of the conducting sphere, and the distance of the generating metal, or the depth and resistance of the electrolyte, are, in fact, the variable conditions in a voltaic combination upon which its efficiency depends; and their relations require further investigation before we shall be able to determine what may be the proper proportions for the economical application of the power to useful purposes. I shall venture, therefore, to trouble you with the results of some further experiments upon the subject, and upon different combinations of the constant battery, before I proceed to communicate some observations upon Electrolysis, which I trust you will find not without interest, and to which, according to my plan, my attention has been lately exclusively directed.

Looking, for a moment, upon the affinity which circulates in the battery as a radiant force, it seemed desirable to ascertain what would be the result of intercepting the rays by the conducting surface nearer to their centre than in the arrangements which have been previously described, as the relation of the generating and conducting metals to each other might be thereby more clearly ascertained.

For this purpose I constructed a battery of ten cylinders of copper, nineteen inches in length by 1½ inch in diameter. As the quantity of acid and sulphate of copper which these cylinders could contain was but small, it was necessary to provide for the perpetual renovation of the charge as the zinc became dissolved and the copper precipitated. This was effected by connecting all the membranes which held the acid and the zinc rods, at their lower ends, with a pipe terminating in a stop-cock. Their upper ends were fixed in a cistern, from which they could be gradually supplied with fresh acid as the saturated acid flowed out below. All the exterior cells formed between the membranes and the copper cylinders also terminated below in a common reservoir, surrounding the former pipe, by which their exhausted contents could be drawn off, as a fresh supply of sulphate of copper was furnished from above by an exterior cistern holding the solution. When the battery was in action, a saturated solution of sulphate of copper, thus supplied from the top, could be drawn off almost colourless at the bottom; and it remained constant for a considerable time.

The effect produced was 4½ cubic inches of mixed gases per minute, measured in the voltameter formerly described.

I compared this result with that obtained from a battery of ten cylinders, twenty inches in length and 3½ inches in diameter, which gave in the same voltameter eleven cubic inches per minute. The surfaces of the conducting cylinders were respectively 89·5 square inches and 220 square inches; and the products of gas in equal times were nearly in the same proportion as the surfaces. The zinc rods were ½ inch in diameter in both cases.

I next proceeded to compare two small hemispheres of copper with the large hemisphere of brass of 9½ inches diameter, formerly described*. They were fitted up, as before, as single circuits, with a zinc ball of one inch diameter, first placed in a membrane below the surface of the solution of copper. The measure made use of was the calorific galvanometer; the first was four inches diameter, and produced a permanent effect of 45° upon the thermometer. The second, of 2½ inches diameter, produced an effect of 29°. The effect of the large hemisphere was 90°. Here it will be observed that the action was by no means proportioned to the surfaces of the conducting hemispheres, but approximated more nearly to the simple ratios of their diameters.

Hence it would appear that the circulating force of both a simple and compound voltaic circuit increased with the surface of the conducting plate surrounding an active centre; but the experiments are not sufficient to determine the law of the increase.

I now constructed a battery of ten larger cylinders, of four inches diameter, the arrangement in everything being the same as before, and found that the action was reduced to one half, the amount of mixed gases in the voltameter per minute being only 5½ cubic inches. The experiments were repeated several times, and the action maintained for several hours, and always with consistent results. This extraordinary and sudden decline of force requires further investigation; and indeed the only conclusion which we can at present draw from the experiments which I have just detailed, but which is of considerable practical importance, is, that cylinders of 3½ inches diameter form much more effective conducting plates in a voltaic arrangement than cylinders of either greater or less diameter. It must, however, be borne in mind that this has only been proved with a series of ten cells; for it is highly probable that the limits of efficiency may change with the number of the series.

The following experiments with the different combinations which may be made with twenty cylinders, throw some light on the question of the influence of the numbers of a series, the diameter of the members of which is limited to 3½ inches. The cylinders were twenty inches in height, and the zinc rods were ⅝ths of an inch in diameter. The electrolyte consisted of eight parts of water and one part of oil of vitriol by measure, and in the exterior divisions of the cells was saturated with sulphate of copper. The temperature was about 65°; the duration of each experiment was one minute.

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* Philosophical Transactions, for 1838, p. 41.

First Set of Experiments.

Number of cells .	1	2	3	4	5	10	15	20
Cubic inches of gas	0	just visible	1⅛	3⅞	6	12¼	15⅓	17¼

Second Set of Experiments.

	All cells direct.	1 inverted.	2 invert.	3 invert.	4 invert.	5 invert.
Cubic inches of gas .	17½	15½	12¾	10½	8½	5½
	6 invert.	7 invert.	8 invert.	9 invert.		
Cubic inches of gas .	3½	1⅝	1⅛	just visible.		

Third Set of Experiments.

Number of double cells	5	10
Cubic inches of gas	11	20

Fourth Set of Experiments.

	5 triple cells.	5 quadruple cells.
Cubic inches of gas	14½	15⅔

When a series of five single cells was connected with a series of five double cells, and the same voltameter employed, the amount of gas was 14½ cubic inches.

When two series of five double cells were each connected with the same voltameter, the amount of gas was 15⅔ cubic inches. Each double series alone gave eleven cubic inches.

From these experiments it appears that the most advantageous adjustment of active force and resistance is in the series of ten single cells when they are of the diameter of 3½ inches, and that the largest amount of work which can be derived from twenty such cells is when they are arranged in two series of ten; for

Cells.	Cubic in.	Cells.	Cubic in.	Cells.	Cubic in.
10 give	12½	5 give	6	4 give	3⅞
	2		4		5
20 . . .	25	20 . . .	24	20 . . .	19⅜

Twenty in single series give 17½ cubic inches. Ten double cells give 20 cubic inches.

Cells.	Cubic inches.	Cells.	Cubic inches.
5 double give	11	5 quadruple give	15⅔
	2		
20	22	20	15⅔

I now combined in a single series a battery of seventy cells of the same dimensions, and charged in the same manner, for the purpose of observing chiefly the effects of light and heat produced by the current in a state of high intensity and constant action. The interior cells were formed of light porous earthenware, and answered their purpose perfectly, offering no more obstruction to the current than the membranes, and being much more convenient in use.

The quantity of decomposition per minute from a voltameter, with the usual charge of dilute acid, was only seventeen cubic inches; while with distilled water the passage of a current was scarcely indicated by a few bubbles of gas which rose from the electrodes.

The flame between charcoal points was of considerable volume, and formed a continuous arch when the points were separated to about three-fourths of an inch. This striking distance did not appear to be increased in a flask exhausted by the air-pump. The light and radiant heat were most intense, and proved highly injurious to the eyes of many of the party who did me the honour of assisting me in my experiments. In my own case, although protected by dark grey glasses of double thickness, a high state of inflammation was produced, which it required very active medical treatment to subdue; and, as you well know, in others, even the application of leeches was found to be necessary. The whole of my face was also scorched and inflamed as if it had been exposed for many hours to a bright midsummer sun. When reflected from an imperfect parabolic metallic mirror in a lantern, the rays were collected into a focus by a glass lens, and readily burned a hole in paper at many feet from their source. The heat was quite intolerable to the hand when held near the lantern.

Paper steeped in nitrate of silver, and afterwards dried, was speedily turned brown in the light; and when a piece of fine wire-gauze was held before it, the pattern of the latter appeared in white lines, corresponding to the parts which it protected.

The phenomenon of the transfer of the charcoal from one electrode to the other, which I believe was first remarked by Dr. HARE, was abundantly produced. The transfer took place from the zincode (or positive pole, or charcoal connected with the last copper cylinder of the battery) to the platinode (or negative pole, or charcoal connected with the last zinc rod of the battery*), and in the former a sharp, well-defined, cup-like cavity was produced, and on the latter, a corresponding protuberance or nipple. The carbon of the latter proved to be very hard, and had the rough mammillated structure of the carbon which is found coating the interior of gas retorts. When a platinum rod was substituted for the charcoal at the platinode, the transfer of the charcoal from the zincode still took place, and the metal became coated with carbon, which was beautifully moulded to its extremity. When this arrangement was reversed, and the zincode was formed of platinum, and the platinode of charcoal, particles of platinum were transferred, and the charcoal became covered with distinct and numerous globules of the fused metal.

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* I have so strongly felt the want of some distinctive names for the two poles of the battery consistent with the principles of nomenclature which you have adopted, that I have ventured to propose those mentioned above, and have constantly used them in my lectures. The *anode* and *cathode* have relation to the surfaces of the electrolyte, and if we distinguish the electrodes themselves as the *anelectrode* and the *cathelectrode*, confusion is apt to arise; I find practically that students easily recollect that the *zincode* is that electrode which in the regular battery would be constructed of zinc, and the *platinode* of platinum.

The transfer of matter of such dissimilar kinds in this definite direction, renders it probable that it is essential to the disruptive discharge, and is entirely analogous to the transfer of matter which has been observed by FUSINERI in all cases of the Leyden discharge, and of the discharge of atmospheric electricity by lightning.

As connected with this subject I may recall to your recollection the attempts which we made to produce a spark before contact, and in which we reduced the distance between two platinum wires connecting the two ends of the battery, to the utmost possible degree, without success. Even when the wires were heated in the flame of a blow-pipe no discharge was established. At the suggestion of Sir JOHN HERSCHEL, I adjusted two brass balls connected with the two ends of the battery, within a very minute distance of each other, and, while in this situation, I passed the spark of a small Leyden jar between them, and immediately the battery current was established, and the brass balls, which were hollow, were burned. Is it not probable that the Leyden discharge in this case transferred the conducting matter which was essential to the existence of the voltaic flame, and which was afterwards supplied by its own energy?

The arch of flame between the electrodes was found to be attracted or repelled by the poles of a magnet according as one or other pole was held above or below it, as was first ascertained by Sir H. DAVY; and the repulsion was at times so great as to extinguish the flame. When, according to the suggestion of Mr. GASSIOT, the flame was drawn from the pole of the magnet itself included in the circuit, it rotated in a very beautiful manner*. When the zincode was connected with the marked pole, and the platinode was held over it, the rotation was from west to east, or in the contrary direction to the motion of the hands of a watch; but when the arrangement was reversed, and the zincode was connected with the unmarked pole, the rotation was reversed. The flame was also made to rotate by the induction of the magnetism of the earth upon a poker of iron held in the direction of the dip.

The experiment was again varied by leading the current through a spiral, twisted round a horse-shoe bar of soft iron, and causing the flame to rotate under the influence of its own magnetic force.

The heating power of the battery was very great, and the greater intensity of the heat on the side of the zincode than on that of the platinode extremely remarkable. Mr. GASSIOT first pointed out to me, that when two stout copper wires, of $\frac{1}{5}$th of an inch in diameter, were connected with the extremities of the battery and held across each other, so that the flame passed between them, the wire at the zincode became red hot, while the other remained comparatively cool. A bar of platinum, ⅛th of an inch square, freely melted and dropped in large globules in the former situation, but showed no signs of fusion at the platinode.

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* This modification of Sir H. DAVY's experiment was first made, as I am informed, by Mr. STURGEON.

When the zincode was formed of the hard carbon taken from the gas retorts, and a cavity ground in it, the most infusible metals placed in it were melted in considerable quantities.

Pure rhodium immediately ran into a perfect globule, and burned with scintillations and a blue light. The native alloy of iridium and osmium, as well as pure iridium, were also completely melted. These metals were kindly supplied to me for the experiments by Mr. JOHNSTON.

Titanium fused instantly, and burned with scintillations very much resembling those from iron.

The native ore of platinum was completely fused; but the mass, when cold, proved to be very brittle.

After four hours constant action, the power of the battery was found to be undiminished, and the amount of the zinc consumed was very small.

In conclusion, I shall briefly describe the results of some experiments on the evolution of the mixed gases from water in a confined space, and under consequent high pressures, which I made from July to October 1837, and which I intended to have further extended. My objects were, to ascertain, 1st, in what manner conduction would be carried on, supposing that the tube in which the electrodes were introduced were quite filled with the electrolyte, and there were no space for the accumulation of the gases; 2ndly, whether decomposition having been effected, recombination would take place at any given pressure; and, 3rdly, whether any reaction on the current-force of the battery would arise from the additional mechanical force which it would have to overcome.

The first apparatus which I made use of was a stout glass tube, into the lower end of which a platinum wire was inserted to form an electrode. This end was hermetically closed, and the upper end ground and fitted with a platinum valve pressed upon by a lever, which could be loaded with weights to any required amount. From this valve a wire projected into the tube to form the other electrode. The tube was accurately filled with the standard dilute acid, and placed in the battery circuit with a voltameter, by which the rate of work and the quantity of the gases disengaged could be ascertained. The battery made use of consisted of ten large cells with the usual charge. Before pressure was applied, the rate of work was always ascertained with the tube and voltameter in their places. I tried many experiments with this arrangement; but it will only be necessary to describe the general results.

The pressure was carried up to 98 lbs. upon a circular area of ¾ inch diameter, the apparatus appeared to be quite tight for a long time, and bubbles of gas were evolved from the two wires when the circuit was complete. The liquid became hazy, and bubbles of gas seemed to line the tube. The stream of oxygen from the upper wire was projected downwards into the liquid, as if with considerable force. The liquid ultimately oozed out between the edge of the tube and the valve, and the experiment was stopped. When the pressure on the valve was removed, a puff of gas took place and the liquid slowly effervesced for a considerable time, but was not projected with any violence. The compression tube felt warm to the hand but not very hot. The quantity of gas which first escaped seemed to bear but a small proportion to that which was indicated by the voltameter included in the circuit, and the rate of decomposition was not at all altered by the accumulation of the elastic force.

To carry the experiment as far as the resistance of glass could conveniently admit of, I caused a compression tube to be made of $\frac{1}{8}$th inch in thickness, of the capacity of $1\frac{3}{10}$th cubic inch. Two platinum plates were sealed into its lower end; one cubic inch of standard acid was poured into it, and it was then hermetically sealed at the top. It was placed securely in the battery circuit with a voltameter, and the progress of the experiment was watched from a safe distance.

The evolution of the gas, which was measured at short intervals, took place with perfect regularity, and did not appear to be in the slightest degree affected by the gradually increasing compression. In $4\frac{1}{2}$ minutes, when nineteen cubic inches had been collected, the compression tube burst with a loud explosion, and the fragments, which were very small, were scattered all over the laboratory.

If we were to calculate that nineteen cubic inches were compressed into the $\frac{3}{10}$ths of a cubic inch space, unoccupied by the liquid, this would be a compression of sixty-three into one, and the pressure would amount to nearly 940 lbs. upon the square inch; but if we were to reckon, as was probably the case, that two cubic inches of the gases were kept down by the solvent power of the liquid at this high pressure, then the compression would have amounted to fifty-six into one, and the pressure to 840 lbs. upon the square inch.

It is probable that the means here pointed out might be applied with advantage to the compression of some of the gases whose liquefaction you have already effected; and I purpose, when my avocations will permit, to return to the experiments with this view.

I remain, my dear FARADAY,
Your faithful friend,
J. F. DANIELL.

King's College, London,
April 9th, 1839.

VII. *On the Electrolysis of Secondary Compounds. In a Letter addressed to* MICHAEL FARADAY, *Esq. D.C.L. F.R.S., Fullerian Prof. Chem. Royal Institution, &c. &c. &c.*

By J. FREDERIC DANIELL, *Esq. F.R.S., Prof. Chem. in King's College, London.*

Received May 15,—Read June 13, 1839.

MY DEAR FARADAY,

I HAVE no doubt that you will agree with me in thinking that the decomposition of secondary compounds by the voltaic current, particularly in connexion with water, has not yet received all the attention which it deserves, and that the subject is worthy of further experimental research.

When water is present in an electrolyte, you have yourself remarked[*] that it is probable that it is always resolved into its first principles; and, on the other hand, the early experiments of Sir H. DAVY prove that when saline substances are present in water, even in the minutest quantities, they are also separated into their elements, or into their proximate principles. Whether these simultaneous decompositions bear any relation to each other, has never, that I am aware of, been made the object of inquiry.

Your discovery of definite electro-chemical action has rendered it of great importance to ascertain, in the case of the decomposition of a saline solution, what proportion of the current may be carried by the oxygen and hydrogen of the water, and what by the acid and alkali, or non-metallic and metallic element, of the salt; and whether there be any definite connexion between the two electrolytes so decomposed. This question was the origin of the following investigation, the results of which have disclosed views of the nature of compound electrolytes and of secondary chemical combinations, which I trust may not be found unworthy of the attention of the Royal Society.

The power which I employed in the following experiments was that of a small constant battery of thirty cells six inches in height, with tubes of earthenware charged in the manner which I have formerly described; and I cannot but remark that without such an instrument the investigation could not have been carried on. The battery was in action almost daily for more than two months, generally from five to six hours per day, and the copper cylinders have thereby become considerably thicker and heavier. Neither the expense nor the uncertain action of batteries of the old construction would have admitted of such an use.

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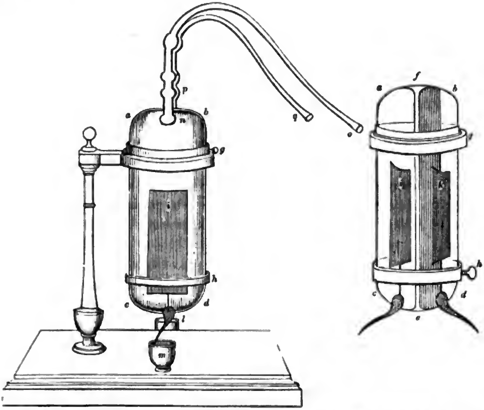
[*] Experimental Researches in Electricity, § 671.

The experimental cell which I first employed consisted of a stout glass cylinder, capable of holding about fourteen cubic inches of liquid, which was originally closed at both extremities. It was cut longitudinally into two equal parts, for the purpose of inserting between the halves a thin plate of fine porous earthenware, which, when the whole was again clamped together by brass rings with screws, divided it into two compartments. The wire of a platinum electrode, 2¾ inches in length and one inch in width, passed through the bottom of each compartment; and to the top of each was ground a bent glass tube for the purpose of collecting the gases evolved during the experiments. The following cut represents a front and side view of the apparatus.



a b c d is the glass cylinder; *e f* the porous diaphragm; *g h* the brass rings and screws by which the two halves are clamped together; *i* and *k* are the two electrodes; *l* and *m* the mercury cups by which the connexions are made with the battery; *n o* and *p q* are the bent tubes by which the gaseous products are collected.

In a preliminary trial one of the compartments was filled with distilled water, and remained for twenty-four hours without penetrating to the other in any appreciable quantity.

A mixture of sulphuric acid and water was prepared beforehand for the purpose of ascertaining in the usual way, by the alkalimeter tube, the amount of any alkaline matter which it might be employed to neutralize; and a similar solution of carbonate of soda for measuring acids. In calculating the results of the experiments, 70·8 cubic inches of mixed hydrogen and oxygen gases have been taken as equivalent to nine grains of water, or one equivalent upon the hydrogen scale expressed in grain weights; and to facilitate computation and comparison, as well as to obtain quantities which might be certainly measured, the experiments were generally continued till the quantities of gases collected indicated a half or a whole equivalent.

Experiment 1.—The cell was charged with a solution of sulphate of soda of the specific gravity 1052, so as to cover the electrodes and to fill about half its capacity. When connexion was made with the battery it was found to conduct well; and the decomposition was allowed to proceed till twenty cubic inches of hydrogen had been collected from the platinode, and nine cubic inches of oxygen from the zincode.

The platinode solution was drawn off carefully with a glass syphon, and found to be strongly alkaline, and to contain by the alkalimeter 12 grains of free soda. The zincode solution was very acid, and neutralized carbonate of soda equivalent to 15·1 grains of sulphuric acid.

The results, therefore, of even this first experiment evidently indicate that the decomposition of an equivalent of water was accompanied by the decomposition of an exact equivalent of sulphate of soda, for the differences are but of inconsiderable amount. I shall not dwell upon the want of exact correspondence between the oxygen and hydrogen, for these are well understood; but taking the amount of the mixed gases as corrected for pressure and temperature at 28·3 cubic inches, we have the following proportions.

Cubic in.		Cubic in.		Equiv. of soda.		Soda.
70·8	:	28·3	: :	32	:	12·8
				Equiv. of sulph. acid.		Sulph. acid.
70·8	:	28·3	: :	40	:	16·1

The experimental results of 12 soda and 15·1 sulphuric acid are not more below the calculated results than might have been expected from the mode of experimenting. It must also be observed, that the level of the solution in the two divisions of the cell altered very much during the experiment; and at its termination the liquid stood 1½ inch higher at the platinode side than at the zincode.

These exact equivalent results are by themselves very remarkable; but I was now anxious to ascertain whether the power of the current were equally divided between, what had hitherto been considered to be, the true equivalent electrolytes.

Experiment 2.—The last experiment was repeated with the same solution of sulphate of soda; but a voltameter, whose electrodes were of the same dimensions as those of the experimental cell, charged with the standard mixture of sulphuric acid and water, was included in the circuit. The experiment was carried on till 70·8 cubic inches of mixed gases had been collected from the voltameter, when it was found that the hydrogen from the platinode of the experimental cell was 47·5 cubic inches, and the oxygen from the zincode 20·25 cubic inches. The former is almost exactly equal to the hydrogen indicated by the voltameter, while the latter is a little short of the equivalent proportion of oxygen. There can, however, be no doubt that the quantities of mixed gases from the saline solution and from the dilute sulphuric acid were equal. Free acid and alkali were found at the zincode and platinode respectively, as in the first experiment, but owing to an accident were not neutralized.

Now if we regard, in the usual way, the convection of the current as effected in the voltameter by the transfer of the oxygen and hydrogen alone, we appear at first to be led to this extraordinary conclusion, namely, that the same current, which is just sufficient to separate an equivalent of oxygen from an equivalent of hydrogen in one vessel, will at the same time separate an equivalent of oxygen from hydrogen, and an equivalent of sulphuric acid from an equivalent of soda, in another vessel. The clearing up of such a result must obviously be of the first importance.

As secondary objects of interest in this experiment, it may be observed, that the temperature of the liquid in the experimental cell rose to 130° Fahr., while that of the acid in the voltameter did not exceed 67°, the quantity of the former considerably exceeding that of the latter; and that the transfer of liquid from the zincode to the platinode occurred as before, so that at the end of the experiment the level of the latter was considerably above that of the former.

The second experiment was repeated twice, and both times the disengaged acid and alkali were neutralized, and found to be in equivalent proportion to the oxygen and hydrogen given off by the electrodes of the experimental cell; which again were together equal to the mixed gases simultaneously given off by the attached voltameter. When the process was continued too long, the proportions of acid and alkali fell short of that of the gases, and that in proportion to the time of its continuance. This was doubtless owing to the gradual mixing of the liquids on the two sides of the diaphragm, and the consequent recombination of the acid and alkali. In every case, transfer of the liquid took place from the zincode to the platinode, and the temperature of the experimental cell rose far above that of the voltameter.

Experiment 3.—The experimental cell was charged with sulphate of potassa of the specific gravity 1069; and a similar cell with dilute sulphuric acid of the specific gravity 1150, coloured on the zincode side with indigo. Both were included in the circuit. After forty minutes action, the saline solution had risen on the platinode side one-fourth of an inch, and fallen to the same amount on the zincode side, while no transfer of liquid had taken place in the acid cell, nor had any colouring matter passed from the zincode side of the diaphragm to the platinode. The indigo, however, had entirely lost its blue colour and become yellow. The amount of mixed gases from both cells was nearly equal; but the oxygen from the acid fell a little short of that from the salt, in consequence, probably, of its absorption by the indigo. The acid disengaged at the zincode and the alkali at the platinode of the saline solution, were also found to be in equivalent proportions to each other and to the gases.

Experiment 4.—A solution of nitrate of potassa of the specific gravity 1117 was substituted for the solution of sulphate of potassa in the last experiment; five cubic inches of oxygen were given off by the zincode, and 4·3 cubic inches of hydrogen only from the platinode. Upon opening the apparatus, a strong smell of ammonia was evolved from the platinode side; evidently indicating a secondary action of the hydrogen upon the nitric acid of the nitre, and accounting for its deficiency. After driving off the ammonia by heat the solution was still alkaline, and by neutralization indicated 13·5 grains of potassa. The neutralization of the acid on the zincode side indicated 17 grains of pure nitric acid. These numbers do not differ much from equivalent proportions, which, upon the assumption that the determination of the acid was correct, would be 17 nitric acid, and 15·1 potassa; or, preferring the determination of the alkali, would be 15 nitric acid, and 13·5 potassa. The quantity of water simultaneously decomposed we can only, in this instance, estimate from the quantity of oxygen collected, which, as we have seen, generally falls below its due proportion; but still it comes near enough to the equivalent proportion to assure us that this would have been the correct determination for

Cubic inches.		Equivalent of nitric acid.		Cubic inches.		Nitric acid.
70·8	:	54	: :	15·0	:	14·2

In this experiment, less of the solution was carried from the zincode to the platinode, and the difference of the level did not exceed $\frac{2}{10}$ths of an inch.

Experiment 5.—The experiment was repeated with the substitution of a solution of phosphate of soda of the specific gravity 1057 for the nitrate of potassa, and the results left no doubt that the simultaneous decompositions of the salt and the water were in equivalent proportions as before. In this experiment, a larger quantity of the solution was carried from the zincode to the platinode than in any of the preceding instances, and the difference of the level on the two sides of the diaphragm amounted to two inches. .

Before I proceed with the principal object of this inquiry, I will make one or two remarks upon this extraordinary transfer of matter from one electrode to the other without the decomposition of the transported compound. It was first observed by Mr. PORRET*, who found it to take place in a glass cell divided into two by a diaphragm of bladder. Into each compartment, filled with water, he plunged a platinum plate and connected the two with the two extremities of a voltaic battery of eighty couples, and nearly the whole of the liquid in the positive cell was carried into the negative cell. It was afterwards found that this phenomenon did not take place when the conducting power of the water was improved by the addition of sulphuric acid.

M. BECQUEREL has also shown† that when finely-divided clay is placed about the water at the zincode in a tube separated from the platinode by a porous diaphragm, the particles of clay are carried forward by the current which is established. This transfer he has also observed to take place only when the water conducts badly.

I charged the experimental cell with the porous diaphragm with distilled water, and found, with the battery of thirty cells, that a few bubbles of gas formed upon the electrodes, but that none was disengaged: in forty minutes, however, the liquid in the platinode cell stood half an inch higher than that in the zincode. This experiment was repeated, a little fresh precipitated alumina having been diffused in the water on the zincode side: a portion of this finely-divided solid matter evidently passed with the water which was transferred to the platinode side.

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* Annals of Philosophy, July 1816.

† Traité de l'Electricité, tom. iii. p. 102. par M. BECQUEREL.

When the water was saturated with boracic acid, its conducting power was a little improved; but still no measurable quantity of gas was given off, and in forty minutes the difference of level in the two compartments of the cell was $\frac{2}{10}$ths of an inch. When the cell was charged with a mixture of eight parts water and one part sulphuric acid, no change of level took place on the two sides.

Notwithstanding the good conducting power, however, of the saline solutions, we have seen that this passage of liquid from the zincode to the platinode occurs with them even to a greater extent than with pure water, and the different species of salts seem to be acted upon in different degrees.

To ascertain whether, during this process of convection, any separation took place of the salt from its solvent, I took the specific gravity of the solutions both before and after the process, and found the differences so trifling as evidently not to arise from this cause. Neither does this large transfer of matter, which is always in the same direction, seem to interfere with the definite electrolytic effect of the current by which it is produced. This will be shown still more strikingly from the subsequent experiments. Whatever may be its proximate cause, the phenomenon appears to be analogous to the transfer of good conducting matter which takes place from the zincode to the platinode during the disruptive discharge of the battery in air, which I referred to in my last communication *, and in which I pointed out to you that the platinum of the zincode was carried forward and deposited upon the charcoal of the platinode. I am also disposed to think that it is quite distinct from the process which M. DUTROCHET has named *endosmose* and *exosmose*, which may be satisfactorily explained by the force of heterogeneous adhesion, without reference to electrical currents.

I have already stated that the products of electrolyzation cannot be kept long separate in the cell which I have described, on account of the ultimate mixture of the liquids on the platinode side of the diaphragm; I was, therefore, led to construct an apparatus which answers this purpose much more perfectly, and which, for distinction, I shall call the double diaphragm cell. It consists of two cells formed of two glass cylinders, with collars at their lower ends, fitted by grinding to a stout glass tube bent into the form of the letter U, and firmly fixed on a wooden foot. The ends of this piece project a little into the interior of the two cylinders, the upper extremities of which are furnished with bent tubes for the collection of gases. A stout piece of platinum wire is ground into the upper part of each cell, to which an electrode of platinum or any other metal can be screwed on the inside, as occasion may require: the wires pass down upon the outside, and terminate in two mercury cups, by which connexion can be made, at pleasure, with the battery. Each cell will hold about seven cubic inches of liquid, and the connecting tube two cubic inches. When the cell is charged, the connecting tube is filled with the liquid, and a piece of fine bladder tied over each end, so as perfectly to exclude the air. The bladders are firmly confined to their places by means of circular grooves ground round the ends of the glass tube. The cylinders are then carefully fitted to their places, and filled with the proper quantities of the solutions to be acted upon, and after the operation their contents are easily decanted, by means of a small glass syphon.

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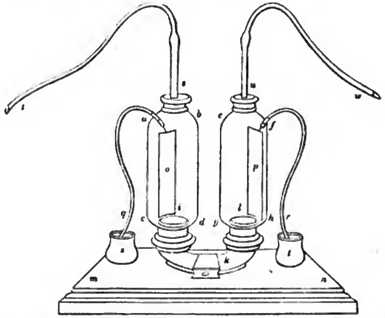
* Present Volume, p. 92, *et seq.*

The apparatus is delineated in the following cut.



a b c d and *e f g h* represent the two glass cells; *i k l* is the bent glass tube fixed in the wooden stand *m n*; *o* and *p* are the electrodes connected with the mercury cups, *s* and *t*, by the wires *a q* and *f r*; *s t* and *u w* are the bent tubes for the collection of the gases evolved.

The current when transmitted by this double cell was, of course, much more retarded than by the single cell, on account of the greater distance of the electrodes, but it answered its intended purpose of stopping the transfer of the liquid even in the case of saline solutions, and there was still conducting power enough to render it perfectly effective. The bladders, however, of the connecting tube generally became a little cupped; indicating a pressure from without, owing to a transfer of a small portion of the liquid within.

Experiment 6.—The two cells were charged each with 4½ cubic inches of a saturated solution of sulphate of soda, and the connecting tube was also filled with the same, and in 2¼ hours the hydrogen evolved from the platinode was twenty-four cubic inches, and the oxygen from the zincode ten cubic inches, making a total of the mixed gases of thirty-four cubic inches; or, making allowance for the deficiency of oxygen, about half an equivalent. After the experiment, the quantities of liquid in the two cells were found unchanged. The zincode solution indicated by saturation the existence of 19·2 grains of free sulphuric acid, and the platinode solution 13·5 grains of free soda, to which must be added two grains of soda in the connecting tube; making a total of 15·5 grains of soda. These numbers are almost identical with the half equivalents of sulphuric acid and soda.

This and similar experiments were frequently repeated with perfectly consistent results; but it will be unnecessary to occupy your time with their details, as I consider that I have already adduced sufficient evidence to prove, that, in the electrolysis of a solution of a neutral salt in water, a current which is just sufficient to separate single equivalents of oxygen and hydrogen from a mixture of sulphuric acid and water will separate single equivalents of oxygen and hydrogen from the saline solution, while single equivalents of acid and alkali will make their appearance at the same time at the respective electrodes.

Experiment 7.—It now seemed to me desirable to ascertain whether in the electrolysis of the dilute sulphuric acid any transfer of the acid took place; and for this purpose, after having taken the specific gravity of the mixture, each cell was charged with five cubic inches of the acid, and the connecting tube also filled with it. In little more than an hour's time after connexion with the battery, the oxygen collected from the zincode amounted to 23·6 cubic inches, and the hydrogen at the platinode to 47·5 cubic inches, or the two gases together to 71·1 cubic inches. The specific gravity of the acid before the experiment was 1126·6. After the experiment, the bulk of the acid both at the zincode and platinode was found unchanged; but, at the former, the specific gravity had increased to 1130·3, and at the latter it had decreased to 1120·3. In a repetition of this experiment, the cells were charged with four cubic inches instead of five cubic inches of acid of the specific gravity 1128·0, and after the collection of the same quantity of the mixed gases, the specific gravity of the acid at the zincode was found increased to 1141·0, and that of the platinode decreased to 1123·0.

From these experiments we learn, that during the electrolysis of an equivalent of water, a portion of acid passed from the platinode to the zincode, and possibly of water from the zincode to the platinode; and, from the greater increase of specific gravity in the smaller quantity of the mixture than in the larger, that the quantities were nearly equal in the two experiments.

But this method did not appear to me capable of determining the point with that precision which its importance rendered desirable; I therefore endeavoured to ascertain the quantity of acid transferred by actually weighing the cells before and after the process.

Experiment 8.—I made use of an apparatus in which the connecting tube covered with its two diaphragms was placed at the top of the two cells, its ends dipping below the surface of the liquid which they contained. The quantity of water decomposed was ascertained by a voltameter included in the circuit. The mixed gases collected amounted to eighteen cubic inches. The weights of the different parts of the apparatus charged with the dilute acid before and after the experiment were as follows:

	Before experiment.	After experiment.	
	grs.	grs.	gr.
Connecting tube . . .	539·2	537·3	1·9 loss.
Platinode cell	2687·8	2685·2	2·6 loss.
Zincode cell.	2631·5	2634·0	2·5 gain.
	5858·5	5856·5	
Total loss		2	
	5858·5	5858·5	

Now if we multiply the amount of the gases by four, the product will be seventy-two, or nearly an equivalent of water decomposed. The total loss, two grains multiplied by four, will be eight grains, which is sufficiently near the equivalent weight of water to lead to the conclusion that the difference arises from errors in experiment: and 2·5 grains, the quantity of acid transferred, multiplied by four, will give ten grains as the amount of sulphuric acid transferred during the electrolyses of one equivalent of water.

This experiment was several times repeated, and the result was always a small gain of weight in the zincode cell, and a loss in the platinode; but I found that I could not rely upon this method of operating for the accurate determination of quantities which it was my object to obtain.

Experiment 9.—I therefore returned to the double diaphragm cell, which I charged with dilute sulphuric acid of different degrees of strength, and having ascertained the quantity of crystallized carbonate of soda which each mixture would neutralize before the experiment, I measured the neutralizing power of each cell after it had been exposed to the battery current. The process was continued in most cases till 70·8 cubic inches of the mixed gases had been collected; and, for the sake of comparison, in the following table, the results have been all brought up to one equivalent by calculation.

Table showing the saturating power of dilute sulphuric acid at the two electrodes for one equivalent of water decomposed.

	1 Acid. 8 Water.	1 Acid. 16 Water.	1 Acid. 32 Water.	1 Acid. 64 Water.	1 Acid. 128 Water.	1 Acid. 8 Water.
Platinode ..	−24	−29	−32	−37	−39	−35
Zincode	+20	+35	+31	+37	+36·5	+36

Now, if we reject the results of the first experiment (for which, however, I have been unable to discover any reason except their inconsistency with the others,) all the rest agree very closely together, notwithstanding the great differences in the mixtures of acid and water from which they were obtained. The mean result also accords very well with the determination at which we had arrived, in Experiment 8, of the transfer of ten grains of sulphuric acid from the platinode to the zincode for every nine grains of water decomposed for

Equivalent of crystallized carbonate of soda.		Equivalent of sulphuric acid.		Carbonate of soda.		Sulphuric acid.
144	:	40	: :	36	:	10

The experiments, however, I conceive, are not sufficient to determine whether the differences of neutralizing power are owing solely to the transfer of acid from the platinode to the zincode, or whether they arise partly from such a transfer, and partly from a simultaneous passage of water from the zincode to the platinode. I have been unable as yet to contrive any experiment to determine this point, which is one of considerable interest.

Experiment 10.—Wishing now to vary the conditions of the last experiment, and at the same time to ascertain whether the same transfer of acid takes place in the active cells of the battery, where the oxygen of the decomposed water is taken up by zinc instead of being given off from a platinum surface, I substituted an amalgamated zinc plate for the platinum in the zincode cell. Its weight before the experiment was 442·5 grains. Each cell was charged with 1123 grains of dilute acid, containing, by calculation from its neutralizing power, 158·8 grains of anhydrous acid. The action of the battery was continued till 23·5 cubic inches of hydrogen had been collected from the platinode, indicating the decomposition of half an equivalent of water. After the experiment, the zinc electrode weighed 426·5 grains, the deficiency being sixteen grains, or half an equivalent of zinc.

The saturation of the liquid at the zincode now indicated 145·1 grains of acid, to which must be added twenty grains for the saturation of the oxide of the half-equivalent of zinc dissolved, making a total of 165·1 grains of acid, and showing an excess of 6·5 grains over the original quantity in the cell.

The saturation of the acid at the platinode indicated 151·8 grains, or a deficiency of seven grains.

This experiment, therefore, leads to the conclusion that about thirteen grains of acid for an equivalent of water decomposed travelled from the platinode to the zincode; but looking to the number, and indeed the uncertainty, of the processes from which the ultimate quantities were deduced, it ought not, perhaps, to be considered as affording more than a general confirmation of the results previously obtained, viz. that a fourth of an equivalent of sulphuric acid passes from the platinode to the zincode for every single equivalent of a compound which has been electrolyzed by the current.

Allow me here to recall to your recollection your own experiments, in which you compared the quantity of acid transferred from the platinode to the zincode from a mixture of sulphuric acid and water, with the quantity transferred by the same current from a solution of sulphate of soda, and found it in one instance as $\frac{1}{35}$th to $\frac{1}{10}$th, or as 0·027 to 0·100, which very little exceeds one fourth, although from another experiment you obtained somewhat more*.

Experiment 11.—The question now arose, Does the acid, during its transfer in the case of the mixed acid and water, or do the acid and alkali in the case of the saline solution, convey any portion of the current which effects the simultaneous decomposition of the water in both instances?

To answer this question the following arrangement was made. The double cell was charged with a saturated solution of sulphate of soda, and connected with it in the circuit was placed a green-glass tube, into the bottom of which was welded a platinum wire forming the platinode to a portion of chloride of lead, which was kept fused in the tube by means of a spirit lamp. The corresponding zincode was formed of plumbago. The platinum wire before the experiment weighed 5·44 grains. The apparatus conducted well, and chlorine escaped in abundance from the plumbago electrode. When 11·7 cubic inches of hydrogen had been collected from the platinode of the double cell, the experiment was stopped and the results examined.

A well-formed button of lead was found adhering to the platinum wire; and the two together, after having been thoroughly freed from the adhering chloride, weighed 27·1 grains, making the reduced lead 21·66. This is about 4½ grains deficient of the quantity which, adopting the chemical number of lead, it ought to have been to be in proportion to the quarter-equivalent of water, which the hydrogen showed to have been decomposed. The sulphuric acid at the zincode amounted to 9·63 grains, and the soda at the platinode to 7·5 grains, both approaching, respectively, very nearly to 10 and 8, which are the exact equivalent numbers. No manner of doubt can therefore exist, that the same current which is just sufficient to resolve an equivalent of chloride of lead, which is a simple electrolyte unaffected by any associated compound, into its equivalent ions, produces the apparent phenomena of the resolution of an equivalent of water into its elements, and at the same time of an equivalent of sulphate of soda into its proximate principles.

Without this experiment, it might have been supposed that in Experiment 2. the acid, which we know must have passed from the platinode to the zincode of the voltameter at the time of the passage of the hydrogen and oxygen, carried the force which effected the decomposition of the sulphate of soda in the experimental cell; but we may now be certain that this is not the case. The last experiment was repeated with exactly similar results.

It was obviously of great moment to inquire next into the action of the voltaic current on the aqueous solutions of the chlorides, as the simpler constitution of this class of salts promised to throw some light upon the nature of the electrolysis of secondary compounds.

Experiment 12.—A plate of pure tin, whose weight was 375·8 grains, was made the zincode of the double cell, which was charged in all its parts with a strong solution of chloride of sodium. During the passage of the current, not the slightest particle of gas appeared upon the tin electrode, nor was the slightest smell of chlorine evolved. The experiment was stopped when twenty-four cubic inches of hydrogen had been collected from the platinode. The tin electrode now weighed 346·1, the loss being 29·7 grains, or almost exactly half an equivalent, and corresponding to the quantity of hydrogen evolved. The platinode solution was alkaline and indicated fifteen grains of soda, to which if we add one grain for some soda in the connecting tube, the solution in which was alkaline, we shall have an exact half-equivalent of soda.

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* Experimental Researches, § 529.

Experiment 13.—The last experiment was repeated with the addition to the circuit of a tube containing, as in Experiment 11., fused chloride of lead. The results are stated in the following table, and compared with the exact chemical equivalents:

	Experiment.	½ equivalent calculated.
Hydrogen evolved . .	12·6	11·8
Lead reduced	24·9	26·0
Tin dissolved	16·3	14·6

The solution in the platinode cell was alkaline, but its saturation was omitted.

Now the simple way of regarding the results of this experiment is, to suppose that, for an equivalent of chloride of lead electrolyzed in the first cell, an equivalent of chloride of sodium was decomposed in the second cell; the chlorine of the latter being absorbed by the tin zincode, and the sodium at the platinode reacting upon the water, and giving rise to the secondary result of an equivalent of hydrogen: upon this hypothesis, the current must have been transmitted by the chloride of sodium alone, and no water was electrolyzed.

Indeed, we must lay it down as a fundamental principle, in discussing the results of all these experiments, that the force which we have measured by its definite action at any one point of a circuit cannot perform more than an equivalent proportion of work at any other point of the same circuit: that the current which we have measured by its electrolyses of an equivalent of simple chloride of lead cannot, at the same time, be sufficient to electrolyze an equivalent of chloride of sodium and an equivalent of water at the same electrodes. The sum of the forces which held together any number of ions in a compound electrolyte could, moreover, only have been equal to the force which held together the elements of a single electrolyte, electrolyzed at the same moment in one circuit.

But how shall we apply this principle to the electrolysis of the solution of sulphate of soda and the results of Experiment 11.? Water seemed to be electrolysed; and, at the same time, the acid and alkali of the salt appeared in equivalent proportion with the oxygen and hydrogen at their respective electrodes. We cannot admit that after the decomposition of the water there was any excess of force applicable to the decomposition of the salt; but we must conclude that the only electrolyte which yielded was the sulphate of soda, the ions of which, however, were not the acid and alkali of the salt, but an *anion* composed of an equivalent of sulphur and four equivalents of oxygen and the metallic *cathion* sodium; from the former, sulphuric acid was formed at the *anode* by secondary action and the evolution of one equivalent of oxygen; and from the latter, soda at the cathode by the secondary action of the metal and the evolution of an equivalent of hydrogen.

These electro-chemical considerations are applicable, of course, to many other saline combinations, as I shall hereafter show; and, in the experiments which I have already detailed, lead to the conclusion that, considered as electrolytes, the following change should be made in the chemical formulæ of the several salts employed.

	Chemical formula.	Electrolytic formula.
Sulphate of soda	$(S + 3\,O) + (Na + O)$	$(S + 4\,O) + Na$
Sulphate of potassa . . .	$(S + 3\,O) + (P + O)$	$(S + 4\,O) + P$
Nitrate of potassa . . .	$(N + 5\,O) + (P + O)$	$(N + 6\,O) + P$
Phosphate of soda . . .	$(P + 2\frac{1}{2}\,O) + (Na + O)$	$(P + 3\frac{1}{2}\,O) + Na$

This view leads me to a modification of the opinion which I have hitherto entertained of the decomposition of sulphate of copper in the constant battery, and the electrolysis of salts, the metallic constituent of which is incapable alone of effecting the decomposition of water at ordinary temperatures. I have always ascribed the appearance of the copper upon the platinode to the secondary action of the hydrogen evolved at that point; but the considerations which I have just submitted to you oblige me to consider it as a primary result of the electrolytic action, the electrolytic formula of sulphate of copper not being $(S + 3\,O) + (Cu + O)$, but $(S + 4\,O) + Cu$. The following experiments were made to elucidate the point still further.

Experiment 14.—The double diaphragm cell was charged at the platinode with a saturated solution of sulphate of copper; the connecting tube and the zincode cell were both charged with dilute sulphuric acid of the standard strength, and a voltameter was included in the circuit. The process was stopped when 35 cubic inches of the mixed gases had been collected. The copper which was precipitated upon the platinode weighed 15·5 grains, and the solution in the platinode cell, which was acid, indicated by neutralization with carbonate of soda, 18·8 grains of free sulphuric acid. The results all approach very nearly to exact equivalent proportions, as shown in the following Table:

	By Experiment. Cubic inches.	By Calculation. Cubic inches.
Oxygen and hydrogen	35	35·4
Precipitated copper	15·5	16
Precipitated sulphuric acid . . .	18·8	20

Experiment 15.—The last experiment was repeated with the substitution of a zincode of zinc for one of platinum: the results are shown in the following Table, and their comparison with the exact equivalent numbers.

	By Experiment. Cubic inches.	By Calculation. Cubic inches.
Oxygen and hydrogen	35	35·4
Precipitated copper	16·7	16
Dissolved zinc	16·4	16
Dissolved sulphuric acid . . .	18·8	20

The appearance of the free sulphuric acid at the platinode cell instead of the zincode is very remarkable. According to the principle which I have laid down, the following view must be taken of the results. The convection of the current in the double cell must have been effected by the electrolysis of the compound electrolyte sulphate of copper $(S + 4\,O) + Cu$, and of the simple electrolyte water $H + O$, the charge being carried by one to its point of junction with the other, and there delivered up to it. If, for convenience, we set out with the sulphate of copper, the metal is deposited upon the platinode, and the compound *anion* $(S + 4\,O)$ travels to the acidulated water, but meeting with nothing with which it can combine, the decomposition of the water commences, the hydrogen of which combines with one equivalent of the oxygen of the compound anion $(S + 4\,O)$, and sulphuric acid $(S + 3\,O)$ remains; the current at the same time passes on with the equivalent oxygen of the water, which is either given off by the platinum zincode or absorbed by the zinc.

Another obvious point of great interest was, to ascertain what relations to the current the products of the electrolysis of the salts of ammonia would exhibit, and I proceeded as follows.

Experiment 16.—The double diaphragm cell, with a tin zincode, was charged with a strong solution of muriate of ammonia, and a voltameter was included in the circuit. The gas from the platinode was collected over mercury. The experiment was stopped when 35 cubic inches of mixed gases had been collected from the voltameter.

No gas was given off from the zincode; but the loss of the tin was 30·4 grains. 23·5 cubic inches of hydrogen were collected from the platinode, and the solution of that cell smelled very strongly of ammonia, and, upon neutralization, was found to contain 8¼ grains of the volatile alkali in a free state. The approximation of these numbers to equivalent proportions will be seen in the following Table:

	From Experiment. Cubic inches.	From Calculation. Cubic inches.
Mixed gases from voltameter	35·0	35·4
Hydrogen from platinode	23·5	23·6
Tin	30·4	29·0
Ammonia	8·25	8·5

Muriate of ammonia, therefore, proved to be an electrolyte whose simple *anion* was chlorine, and compound *cathion* nitrogen with four equivalents of hydrogen. Its electrolytic symbol therefore, instead of being $(C^h + H) + (N\,3\,H)$, would be $C^h + (N + 4\,H)$.

Experiment 17.—The double cell was now charged with a strong solution of sulphate of ammonia, and the experiment conducted as before. Thirty-five cubic inches of mixed gases were collected from the voltameter, 11·5 cubic inches of oxygen from the zincode, and 23·0 cubic inches of hydrogen from the platinode. The zincode solution was acid, and the platinode by neutralization showed eight grains of free ammonia.

	By Experiment. Cubic inches.	By Calculation. Cubic inches.
Mixed oxygen and hydrogen	35	35·4
Oxygen from zincode	11·5	11·66
Hydrogen from platinode	23·0	23·32
Ammonia	8·0	8·5
Sulphuric acid		20·0

To explain these results, we must take into consideration that sulphate of ammonia is not, in a chemical point of view, a mere compound of sulphuric acid and ammonia, but that an equivalent of water is essential to its existence. Its formula is $(S + 3 O) + (N + 3 H) + (H + O)$; and by electrolysis it is resolved into one equivalent of sulphur with four equivalents of oxygen given off at the zincode, and one equivalent of nitrogen and four equivalents of hydrogen disengaged at the platinode. As an electrolyte its formula would therefore be $(S + 4 O) + (N + 4 H)$. Both the compound *anion* and the compound *cathion* agree perfectly in constitution with those which we have previously found evolved from their combinations with simple substances, at their respective anodes and cathodes.

It is impossible, I think, not to be struck with the singular, and, by me, perfectly unexpected coincidence of the results which I have just detailed with two celebrated hypotheses; the one of Berzelius, with regard to the constitution of the muriate of ammonia, and the other of Davy, concerning the nature of the aqueo-acids and their saline compounds.

The former has been led to imagine, from analogies which it will be quite unnecessary to recapitulate at present, that muriate of ammonia is a chloride of a hypothetical radical, which he has denominated *ammonium*, and which is constituted of one equivalent of nitrogen and of four equivalents of hydrogen; and the oxide of this radical he considers as the basis of the oxysalts of ammonia. According to this view, muriate of ammonia is represented by the formula $(N + 4 H) + C^h$, and sulphate of ammonia by $(N + 4 H + O) + (S + 3 O)$. The former agrees exactly with the conclusion which we have derived from the electrolysis of the salt; but the latter differs from the electrolytic view, which is not that of a combined acid and base, but of *ammonium* and a compound anion $(S + 4 O)$.

The hypothesis of Davy was, that the salts of the oxyacids might have an analogous constitution to that of the binary compounds of chlorine and metals, and that the aqueo-acids might be regarded as hydro-acids. As muriatic acid, therefore, is a compound of the simple radical chlorine and hydrogen, or $C^h + H$, aqueo-sulphuric acid may be a compound of a compound radical and hydrogen, or $(S + 4 O) + H$.

When muriatic acid is brought to act upon soda, water is formed and chloride of sodium, or $C^h + Na$. When aqueo-sulphuric acid is made to act upon soda, water is also formed, and a binary combination of the compound radical and sodium, or $(S + 4 O) + Na$.

The general view which he propounded was, that a *radical* (which might be either simple or compound, as chlorine or cyanogen, or $(S + 4\,O)$ forms an acid with hydrogen, and a salt with sodium or any other metal. It was supported by many analogies; and it certainly has the advantage of assimilating in constitution a natural group of bodies which resemble one another so closely as the salts, and which former theories separated into the two dissimilar classes of *oxysalts* and *haloid* salts. The progress of organic chemistry, and the doctrine of substitutions, has lately added to the presumptive evidence which favours this hypothesis, and the results of electrolysis which I have just stated will probably be reckoned as direct evidence of its correctness. The only phenomena which do not fall within its comprehension, are those of the decomposition of the dilute sulphuric acid; as there appears no reason why the aqueo-acid should not be resolved into sulphuric acid, and one equivalent of oxygen at the zincode, and hydrogen at the platinode, or $(S + 4\,O) + H$ instead of $\left(\frac{S + 3\,O}{4} + O\right) + H$.

If we regard the water as the electrolyte which yields upon this occasion, it is no less difficult to understand the quarter equivalent of sulphuric acid which accompanies the oxygen to the zincode, and with which the facility of the convection is connected. It is not, however, wholly dependent upon it; for although, as we have seen, the quantity of sulphuric acid which passes is the same in all cases, the facility of electrolyzation decreases as the proportion of acid falls below one part in nine of the mixture.

The formation of these secondary electrolytes, and compound anions and cathions, will probably furnish the key to the explanation of many of those chemical compositions and decompositions to which the presence of water is necessary, such as those of nitric acid with the metals, and to the formation of SCHÖNBEIN's circuit; but the experimental examination of the hypotheses, and of the general question, must be left to a future opportunity. In the mean time,

I remain, my dear FARADAY,
Your faithful friend,
J. FREDERIC DANIELL.

King's College, London,
14th May, 1839.

POSTSCRIPT.

I have since ascertained that in aqueous solutions of the fixed alkalies the oxides travel in the contrary direction to that of the acid in the dilute sulphuric acid, and accumulate at the platinode. The amount transferred is in all cases below that of the equivalent of the gases evolved. I am at present engaged in fixing the exact relative proportions, and hope soon to communicate to you the results of my experiments.

J. F. D.

15th June.

VIII. *On a new Equiatomic Compound of Bicyanide with Binoxide of Mercury. By* JAMES F. W. JOHNSTON, *Esq. M.A. F.R.S., Professor of Chemistry and Mineralogy in the University of Durham.*

Received March 21,—Read April 11, 1839.

WHEN hydrocyanic acid of considerable strength (10 to 20 per cent.) is agitated with red oxide of mercury in large excess, a white compound is obtained, intermixed with the red oxide, on which cold water has very little action. If the mixture be collected on the filter and treated with boiling water, the new compound is separated from the excess of oxide, and, as the solution cools, is deposited on the sides of the vessel in the form of a white incrustation, adhering strongly, and consisting of an aggregation of colourless transparent four sided acicular prisms. In favourable circumstances, I have obtained it in such prisms half an inch in length, but not sufficiently perfect to admit of measurement.

This salt is remarkably distinguished from the bicyanide by its sparing solubility in cold water, by the strong alkaline reaction exhibited by its solution, and by its relations to heat. Heated gently in the air, it blackens slightly and then explodes with little noise, but if it be heated in larger quantity (5 to 10 grs.), and in a close tube, it explodes with a loud detonation, and shivers the tube into fragments. It does not explode under the blow of a hammer.

Burned with bichromate of potash or oxide of copper it gives off a gas which consists of nitrogen and carbonic acid, in the proportion of one volume to two. Thus

1.	131 vols. treated with caustic potash left	47
	239 vols. treated with caustic potash left	79
2.	122 vols. treated with caustic potash left	40
	91 vols. treated with caustic potash left	31·5
	590	197·5

and 197·5 × 3 = 592·5.

The formation of this new salt therefore is not due to any decomposition of the cyanogen.

Dissolved in dilute muriatic acid and gently heated, it emits the odour of prussic acid. If sulphuretted hydrogen be passed through the solution in water, a precipitate, at first white and afterwards becoming black, is thrown down. To the solution filtered and heated to drive off the excess of hydrosulphuric acid, sulphate of iron and afterwards caustic potash were added, when a precipitate of Prussian blue fell. The salt therefore contains

1. Cyanogen, since its solution when decomposed by hydrosulphuric and hydrochloric acid gives hydrocyanic acid.

2. Its alkaline reaction indicates an excess of mercury.

3. That this excess is in the state of oxide is shown by the detonating property of the salt, the sudden decomposition being due to the action of the oxygen on a portion of the carbon of the cyanogen.

Heated to incipient decomposition the salt loses no weight, it is therefore anhydrous.

29·275 grains dissolved in hydrochloric and precipitated by hydrosulphuric acid gave 29·245 grains of bisulphuret of mercury = 86·191 grains of metallic mercury per cent.

29·27 grains gave 29·15 grains bisulphuret = 85·933 Hg per cent.
34·93 grains gave 34·68 grains bisulphuret = 85·674 Hg per cent.

The last result, as we shall see, is nearest the truth.

Burned with oxide of copper,

63·55 grains gave $\ddot{C}$ = 11·96 = 5·203 carbon per cent.
$\dot{H}$ = 0·60

43·94 grains gave $\ddot{C}$ = 8·00 = 5·034 carbon per cent.

The small quantity of water obtained in the first analysis was obviously accidental, and therefore the water was neglected in the second. And since the nitrogen is to the carbon, as already shown, in the proportion of one volume or atom to two, 5·203 carbon, the result of the two very approximate ones, which should be nearest the truth, indicates 6·025 of nitrogen, since,

152·875 : 177·036 : : 5·203 : 6·025.

Therefore the salt consists by analysis of

Carbon . . . . = 5·203
Nitrogen . . . . = 6·025
Mercury . . . . = 85·674
Oxygen . . . . = 3·098
100

This composition agrees with the formula ($Hg\ Cy_2 + Hg\ O_2$) which gives

			per cent. calculated.	per cent. by experiment.
2. Cyanogen	4 carbon =	305·750 =	5·162	5·203
	2 nitrogen =	354·072 =	5·977	6·025
	2 mercury =	5063·288 =	85·483	85·674
	2 oxygen =	200·000 =	3·378	3·098
		5923·110	100	100

This salt exhibits many very interesting reactions.

1. Heated over a lamp, under water, it becomes yellow, and a portion of a yellow powder (Hg Cy ?) ultimately remains undissolved, mixed with protoxide and a little metallic mercury; that is, a portion of it is decomposed, probably according to the following formula

$$(\mathrm{Hg\,Cy_2} + \mathrm{Hg\,O_2}) + 3\,\mathrm{H\,O} = \mathrm{Hg\,Cy} + \mathrm{Hg\,O} + (\mathrm{N\,H_3} + 2\,\mathrm{C\,O_2})$$

the reduction of the mercury being the consequence of a second reaction.

2. Caustic ammonia throws down from a cold solution of this salt, a copious and bulky white precipitate; from a hot solution, the precipitate is slightly yellowish. The supernatant liquid, on evaporation, gives pure bicyanide of mercury. The precipitate when dried at 212° Fahr. and heated in a close tube over a lamp, gives off ammonia, water, and metallic mercury. It is probably the same compound which is obtained by digesting red oxide of mercury in caustic ammonia, and which is *supposed* to be represented by one or other of the formulæ

$$(3\,\ddot{\mathrm{H}}\mathrm{g} + 4\,\mathrm{N\,H_3} + 8\,\dot{\mathrm{H}}) \text{ or } (\ddot{\mathrm{H}}\mathrm{g} + 2\,\overline{\mathrm{Hg} + 2\,\mathrm{N\,H_2}} + 12\,\dot{\mathrm{H}}).$$

3. A solution of sal ammoniac throws down a yellow precipitate, the supernatant liquid having a strong alkaline reaction. On heating, the precipitate is redissolved, ammonia is given off, and the alkaline reaction disappears. The *direct* action here is the change of the binoxide into bichloride and the liberation of ammonia, according to the formula

$$(\mathrm{Hg\,Cy_2} + \mathrm{Hg\,O_2}) + 2\,\overline{\mathrm{N\,H_4\,Cl}} = \mathrm{Hg\,Cy_2} + \mathrm{Hg\,Cl_2} + 2\,\overline{\mathrm{N\,H_4\,O}}.$$

The white precipitate is a secondary result, due to the action of the free ammonia (as above mentioned), and which consequently disappears when the ammonia is driven off by heat. In this case, however, since bichloride is present, the white precipitate will not be the same as is produced by the action of ammonia on the new salt (2.), but may consist in part or in whole of the long-known *hydrargyrum precipitatum album*, the true composition of which $(\mathrm{Hg\,Cl_2} + \overline{\mathrm{Hg} + 2\,\mathrm{N\,H_2}})$ has been recently established by Dr. Kane. This reaction is represented by the formula

$$2\,\mathrm{Hg\,Cl_2} + 4\,\mathrm{N\,H_3} = (\mathrm{Hg\,Cl_2} + \overline{\mathrm{Hg} + 2\,\mathrm{N\,H_2}}) + 2\,\overline{\mathrm{N\,H_4\,Cl}}.$$

4. The fixed alkaline chlorides render the solution of the new salt milky, and throw down a small quantity of a white precipitate, the liquid becoming more strongly alkaline. In this case also, the chloride gives up its chlorine to the mercury, and the free alkali causes the increased causticity of the solution. The milkiness I attribute to the presence in small quantity either of ammonia, or of some neutral salt (formiate ?) of mercury, the decomposition of which by the alkaline chloride causes the formation of an equivalent portion of calomel. It is known how readily formic acid and ammonia are produced at the expense of cyanogen, when water is present*, and it would appear that the action of heat upon this new double salt in water very frequently gives rise to these products. In treating with boiling water the mixture of

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* The reaction is thus represented: $\mathrm{N\,C_2} + 4\,\mathrm{H\,O} = (\mathrm{H\,C_2\,O_3} + \mathrm{N\,H_3})$.

oxide of mercury and this salt, (which is produced on digesting prussic acid with the latter in excess,) in order to obtain the salt in solution, copious bubbles of gas are evolved, the smell of ammonia becomes perceptible, and much metallic mercury is precipitated*. On concentrating the solutions also, decomposition takes place, with deposition of metallic mercury; and when the crystallized salt is dissolved in boiling water, a black sediment in greater or less quantity is generally observable. In the solution therefore when cold, the presence either of a trace of a formiate or of any ammoniacal salt would account for the slight deposit which occurs when chloride of potassium is added to it. When chloride of magnesium is employed, the milkiness is more speedy and more distinct, a light bulky hydrate of magnesia being thrown down, as is to be expected, when we consider that on parting with its chlorine the magnesium is oxidized, and set at liberty in a liquid which can retain very little of it in solution.

If the salt be added in successive quantities to a concentrated boiling solution of chloride of potassium, it is largely dissolved, and on cooling, the beautiful pearly scales of the compound of bicyanide of mercury with chloride of potassium fall down. The supernatant liquid contains bichloride of mercury and caustic potash.

A solution of iodide of potassium produces analogous compounds and reactions, as does also a solution of cyanide of potassium.

5. The solution of this new salt (oxycyanide), as has been already stated, has a strong alkaline reaction. If it be added to a very dilute solution of nitric acid till the solution ceases to redden litmus, it will be found to dissolve with great ease and in large quantity. The solution by careful evaporation gives long, delicate, transparent, colourless, quadrangular prisms, or hexagonal plates and pearly scales, and crystallizes to the last drop.

At 212° FAHR. these crystals lose nothing, and therefore contain no water. Over the lamp in the open air they decompose with a flash of white light, giving off metallic mercury, and leaving a yellow residue. In a close tube they are decomposed with a slight detonation, and with the emission of red fumes.

38·334 grains of the oxycyanide, cautiously treated in this way, gave beautiful crystals, weighing 40·53 grains, equal to an increase of 5·728 per cent. The new compound therefore consists of

		Calculated.	Found.
One equivalent of oxycyanide ($Hg\,Cy_2 + Hg\,O_2$) .	= 59·23	94·285	94·272
One half equivalent of nitric acid, $\frac{1}{2}\,N\,O_5$	= 3·38	5·715	5·728
	62·61	100	100

This salt is readily soluble in water. What is chiefly interesting in regard to its constitution and properties is, that the alkaline reaction of the oxycyanide should be fully destroyed by so small a quantity of nitric acid. This fact would seem to show, that the bicyanide of mercury, and consequently other compounds of the same class, do really possess something of the characters of the true acids, having the power, if not of completely neutralizing a metallic oxide, at least of so far modifying its basic character as materially to weaken its neutralizing action on the oxygen acid.

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* This second reaction is due to the decomposition of the formiate of ammonia by the agency of the binoxide of mercury thus: $(H\,C_2\,O_3 + N\,H_3) + Hg\,O_2 = (N\,H_3 + C\,O_2) + C\,O_2 + Hg$.

6. This is further illustrated by the action of acetic acid on the oxycyanide. A weak solution of this acid dissolves it in large quantity, and if carefully saturated, the solution gives, on evaporation, a white salt having the odour of acetic acid, which loses nothing at 220° Fahr., but when heated in a close tube, blackens and gives off cyanogen, metallic mercury, and a strong odour of acetic acid. In preparing this salt, it is difficult so perfectly to neutralize the acetic acid, that, during the evaporation*, any excess that may be present should not, as it becomes concentrated, decompose some of the bicyanide which the solution contains. On the following results, therefore, I do not place much reliance, though they serve to show by how small a quantity of this acid the oxycyanide may be perfectly neutralized.

Increase.
49 grains of oxycyanide gave 49·94 of the new salt = 1·918 per cent.
34·567 grains of oxycyanide gave 35·36 of the new salt = 2·294 per cent.
54·00 grains of oxycyanide gave 54·92 of the new salt = 1·704 per cent.

Taking the atom of acetic acid at 623, these results indicate the addition of *one-sixth* of an equivalent of this acid to each equivalent of the oxycyanide, or that the new compound is $(\mathrm{Hg\,Cy_2} + \mathrm{Hg\,O_2} + \frac{1}{6}\,\overline{\mathrm{A}})$. Thus we have

		Calculated per cent.	Found per cent.
One equivalent of the oxycyanide	= 5923	= 98·23	98·296
One-sixth equivalent of acetic acid = $\frac{643}{6}$ =	107	= 1·77	1·704
	6030	100	100

Though this is not a solitary instance in which one of acetic acid combines with six of base, the subacetate of lead $(6\,\dot{\mathrm{P}}\mathrm{b\,O} + \overline{\mathrm{A}})$ being an analogous salt, yet I would be understood as representing the constitution above given for this salt as open to future correction.

I have tried also several other organic acids, and have found that with the benzoic and citric acids, soluble and crystallizable salts may be obtained, and with tartaric acid, a compound which, after being crystallized, is decomposed by water into a soluble and an insoluble portion. Oxalic acid gives a white precipitate, and the filtered solution, on evaporation, two salts; one in white prisms, the other in yellowish crystals. I have not subjected any of these compounds to analysis.

7. When an acid solution of nitrate of silver is poured into a hot saturated solution of the oxycyanide, a shower of beautiful pure white prismatic crystals falls.

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* When a solution of the bicyanide is heated with acetic acid the odour of prussic acid becomes distinctly perceptible.

These crystals dissolve readily in hot water and re-crystallize on cooling. Heated in a close vessel they give off water, become opake, melt, and finally explode, with the beautiful purple flame characteristic of burning cyanogen.

Heated on the Water Bath.
33·125 grains lost 2·63 grains = 7·939 per cent.
31·075 grains lost 2·46 grains = 7·916 per cent.

On standing in a moist atmosphere for a few days, the dry salt recovers the whole of its water of crystallization.

The 33·125 grains, after drying as above, were dissolved in water and precipitated by muriatic acid. The chloride of silver obtained weighed 10·48 grains, = 23·834 per cent., or 37·533 of nitrate of silver. This salt therefore is the compound discovered by Wöhler ($Hg\,Cy_2 + Ag\,O\,.\,N\,O_5 + 4\,H\,O$), and which is composed of

		Calculated per cent.	Found per cent.
Bicyanide of mercury, 1 equivalent	= 2991·46	53·709	54·540
Nitrate of silver, 1 equivalent .	= 2128·64	38·214	37·533
Water, 4 equivalents	= 449·92	8·077	7·927
	5570·02	100	100

Many of the other interesting reactions of the oxycyanide may be predicted from its constitution, and its use as a reagent will occasionally be valuable to the chemist, from its affording a solution in which binoxide of mercury exists as such, and possessed of alkaline properties.

Durham, February 1839.

IX. *On the Constitution of the Resins. By* JAMES F. W. JOHNSTON, *Esq. M.A. F.R.S., Professor of Chemistry and Mineralogy in the University of Durham.*

Received March 21,—Read April 11, 1839.

THE object of the investigation, of which the present paper forms a part, is

1. To determine the relative composition of the various resins which occur in nature. Possessing so many properties in common, this large family of natural productions ought also to present many analogies in constitution.

2. To ascertain how far they may be considered as derivatives from one common radical; and

3. Whether it is possible to represent them all by one or more general formulæ.

I. *Resin of Mastic.*

Mastic resin is said to be obtained from the *Pistacea lentiscus,* and to be produced chiefly in the island of Chios. It occurs in drops or tears, which are transparent, and of a pale yellow colour. It melts at 212°, and emits a peculiar and not unpleasant odour. Fused in a retort it gives off an acid liquid in small quantity. If the heat be raised to 300° FAHR. and upwards, the melted mass froths up, and water and acid vapours are evolved. At a higher temperature a pale yellow liquid distils over very slowly, at first of the consistence of oil, but increasing in thickness as the process proceeds, water and acid being also given off during the whole process. What remains in the retort is of a black colour, and nearly insoluble in alcohol.

Digested in cold alcohol, mastic resin is in great part dissolved; what remains is nearly pure white, elastic, capable of being drawn out into long fibres of a silky lustre, and is a compound with alcohol of a second resin (B), very sparingly soluble in alcohol, but more largely in an alcoholic solution of resin A. Hence the solution obtained contains a portion of the second resin, which may be in a great measure precipitated by large dilution with alcohol.

I. The solution of resin A thus obtained is of a pale yellow colour. Distilled and evaporated at 212° FAHR., it gives a pale yellow resin flowing freely at that temperature, but at higher temperatures frothing up and giving off alcohol.

Of a portion fused at 350° FAHR. till it ceased to emit bubbles of vapour,

8·67 grains gave $\ddot{C} = 24{\cdot}685$, and $\dot{H} = 8{\cdot}18$
10·73 grains gave $\ddot{C} = 30{\cdot}335$, and $\dot{H} = 9{\cdot}80$
10·294 grains gave $\ddot{C} = 29{\cdot}16$, and $\dot{H} = 9{\cdot}614$

These results give per cent.

	(1.)	(2.)	(3.)
Carbon	= 78·729	78·172	78·328
Hydrogen	= 10·483	10·337	10·377
Oxygen	= 10·788	11·491	11·295
	100	100	100

These agree nearly with the formula $C_{40}\,H_{32}\,O_4$, which gives

Carbon	= 79·275
Hydrogen	= 10·355
Oxygen	= 10·370
	100

and was adopted by **Rose** as representing the constitution of colophony. The carbon, however, is deficient, and the hydrogen, according to the more recent analyses of **Trommsdorf**, **Liebig**, and **Laurent**, is in larger quantity than in either the pinic or sylvic acid. I therefore dissolved in alcohol a second portion of the mastic of the shops, diluted the solution largely* with alcohol to separate the resin B, evaporated and heated to 260° **Fahr.** for eighteen hours, stirring it occasionally as long as bubbles of vapour were evolved in any quantity.

Of this resin 10·38 grains gave $\ddot{C} = 29{\cdot}785$, and $\dot{H} = 9{\cdot}48$, or

	(4.)
Carbon	= 79·343
Hydrogen	= 10·147
Oxygen	= 10·510
	100

which is almost identical with the result of **Rose** for the crystallized resin of copaiva balsam.

II. Heated again in the air to 350° **Fahr.** it had lost its fragrant odour, and emitted white vapours. In this state 8·718 grains gave $\ddot{C} = 25{\cdot}16$ and $\dot{H} = 8{\cdot}02$ grains. These are equivalent to

	(5.)	$C_{40}\,H_{30}\,O_4$ gives
Carbon	= 79·800	79·79
Hydrogen	= 10·221	9·77
Oxygen	= 9·979	10·44
	100·000	100·000

The carbon in this analysis is the exact theoretical quantity required by **Liebig**'s formula for the sylvic and pinic acids, but the hydrogen is still considerably in excess.

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* The solution from which the resin first analysed was obtained had not been diluted, as I was not then aware that by this means resin B would be more effectually separated. The resin A of analysis 1, 2, 3, might therefore contain a portion of resin B.

The resin thus analysed, however (5.), had undergone a change of constitution by being heated to 330°, for, when treated with alcohol, a large portion of it refused to dissolve, remaining behind as a soft reddish resin, on which the alcohol of the shops appeared to have little action in the cold.

After taking up all that was soluble, the alcoholic solution was evaporated, and the resin heated for two hours to 260° Fahr. It was transparent, reddish yellow, and very beautiful. On analysis 10·965 grains gave $\ddot{C}$ = 31·925 $\dot{H}$ = 10·742 grains, which are equal to

(6.)
Carbon = 80·507 = 40 atoms
Hydrogen = 10·885 = 33·11 atoms
Oxygen = 8·608 = 3·27 atoms
100

The soluble part therefore of the changed resin was no longer pure mastic resin A (see *infrà* analysis, Nos. 26, 27, 28.).

III. A third portion of the mastic of the shops was boiled in water for several hours, by which it was rendered white, opake, and less fusible, probably from the loss of a portion of volatile oil existing in the resin of commerce. It was then digested in a large quantity of alcohol, in which it now also dissolved more slowly. The solution was evaporated, and the resin obtained again boiled in water for a length of time to drive off the whole of the alcohol.

1. A portion of this resin was then heated to about 240° Fahr., when it fused freely and ran smooth, and when cold had the same beautiful colour and appearance as that previously analysed.

10·956 grains gave $\ddot{C}$ = 31·35, and $\dot{H}$ = 10·136, or per cent.

(7.)
Carbon = 79·122
Hydrogen = 10·279
Oxygen = 10·599
100

2. A second portion was heated only to 212° for 48 hours, when it had become transparent and of a pale yellow colour. At this temperature it softened, but did not enter into perfect fusion. It still showed a tendency to form minute bubbles of gaseous matter.

10·713 grains gave $\ddot{C}$ = 30·71, and $\dot{H}$ = 10·02 grains, or per cent.

(8.)
Carbon = 79·265
Hydrogen = 10·392
Oxygen = 10·343
100

The resin employed in these last analyses (7. and 8.) was entirely dissolved when treated with alcohol, No. 7. leaving only a trace of insoluble matter. They may be considered therefore as representing very nearly the constitution of the soluble resin of mastic. The formula $C_{40}\ H_{31}\ O_4$ agrees very closely with these results, and with that of analysis No. 4, thus:

		Calculated.	(7.) Exper.	(8.) Exper.	(4.) Exper.
40 Carbon	= 3057·480	= 79·531	79·122	79·265	79·343
31 Hydrogen	= 386·867	= 10·063	10·279	10·392	10·147
4 Oxygen	= 400·000	= 10·406	10·599	10·343	10·510
	3844·347	100	100	100	100

If this be the true composition, it shows a close approximation to the pinic and sylvic acids, and yet a cause for the difference in the sensible properties exhibited by these different kinds of resin. If such differences should actually be found to exist in nature, the striking fact will at once suggest itself, that under the general expression $C_{40}\ H_{32-x}\ O_4$ we may have a great variety of resins; and a still greater variety, exhibiting also more sensible differences in their physical properties under the more general one of $C_{40}\ H_{32-x}\ O_{4\pm y}$.

IV. *Action of Heat.*—We have seen that when resin A is heated to 260° Fahr. or upwards, it is partly decomposed, and an insoluble portion remains when it is treated largely with common alcohol in the cold. This resin is in mass of a reddish colour, and is soluble in boiling alcohol, from which it falls as a yellow powder on cooling. Dried at 212° Fahr. it does not melt, but at about 130° it begins to cohere into a dark reddish brown mass. Heated for twelve hours at 212°, and afterward exposed to a temperature at which it began to cohere,

A. 8·538 grains gave $\ddot{C} = 24·33$ and $\dot{H} = 7·776$

B. 10·994 grains gave $\ddot{C} = 31·245$ and $\dot{H} = 9·976$

These are equivalent to

	A. (9.)	B. (10.)
Carbon	= 78·762 =	78·943
Hydrogen	= 10·119 =	10·128
Oxygen	= 11·119 =	10·929
	100	100

2. As these results do not give any clear idea of what takes place when the resin is thus heated, I introduced a portion of the pure resin into a glass retort and kept it fused in a sand bath for twenty-four hours, the heat during the last twelve hours being raised to 270° Fahr. Water was deposited in the neck of the retort, rendered sour by a crystallizable acid in small quantity. The heat was withdrawn, when a

pale yellow liquid began to show itself in the upper part of the retort. The resin was red and exceedingly beautiful, and when treated with alcohol was now separable into three portions.

A. Boiled with alcohol of the shops, a portion of a red colour was left, on which this liquid appeared to have little further action. It adhered strongly to the retort, but was separated by boiling water, and obtained in the form of a dirty yellow powder, which, at 270° Fahr., showed no symptoms of fusion. Dried at this temperature

8·475 grains gave $\ddot{C} = 23·625$ and $\dot{H} = 9·859$, or

		(11.)	$C_{40} H_{30} O_5$ gives	$C_{40} H_{31} O_5$ gives
Carbon	=	77·080	77·76	77·51
Hydrogen	=	9·859	9·52	9·80
Oxygen	=	13·061	12·72	12·69
		100	100	100

From a single analysis, which was all that the quantity at my disposal enabled me to perform, it is impossible to determine which of the formulæ represented in columns 2 and 3 is the true one. Probability, however, is in favour of the first, or $C_{40} H_{30} O_5$, which indicates that when decomposed by a moderate heat one of the changes which mastic resin A. undergoes is the substitution in a part of it, of an atom of oxygen for one of hydrogen, the new compound being nearly insoluble in common alcohol even at a boiling temperature.

B. The boiling alcoholic solution deposited on cooling a pale yellow resin, which heated and fused, became brownish red.

11·817 grains gave $\ddot{C} = 33·858$, and $\dot{H} = 10·356$ grains.

C. The cold alcoholic solution, when evaporated and heated gently till perfectly transparent, gave a beautiful reddish yellow resin, of which

12·05 grains gave $\ddot{C} = 34·003$, and $\dot{H} = 11·227$ grains.

These results are equal to

		B. (12.)		C. (13.)
Carbon	=	79·225	=	78·026
Hydrogen	=	9·737	=	10·352
Oxygen	=	11·038	=	11·622
		100		100

From these analyses we cannot derive much information with regard to the constitution of these soluble portions of the changed resin, and it would be necessary to carry the process further than was done in this experiment, before the action of heat can be accurately understood. If we might be guided in our opinions as to the ultimate nature of the soluble part by the constitution of the small quantity of which

No. 6. exhibits the analysis, we should infer that two new resins are produced, in the formulæ for which are found O_5 and O_3 respectively. But the subject requires further investigation, and I hope to be able to return to it at a future period.

V. *Atomic weight of Resin A.* It has been already observed by UNVERDORBEN that the resin of mastic bears a considerable resemblance to colophony, and combines with nearly the same proportion of oxide of lead.

A. *Salts of Lead.* When a solution of this resin in alcohol is mixed with an alcoholic solution of acetate of lead, a white precipitate falls, which, when dried at 212° becomes yellow; at 350° FAHR. it melts into a brownish yellow mass without decomposition.

1. Dried at 300° FAHR. 11·610 grains left 2·307 grains of oxide of lead = 19·878 per cent.

2. Of the same *fused* at 350° FAHR., 10·33 grains left 2·07 grains of oxide = 20·038 per cent.

These give for the constitution of the salt

	1.	2.	
Resin . . .	80·122	79·962	. . . . (14.)
Oxide of lead .	19·878	20·038	
	100	100	

It is therefore a *sesquisalt* = $1\frac{1}{2}(C_{40}H_{31}O_4) + PbO$, which consists of

$$\left.\begin{array}{l}\text{Resin} = 80{\cdot}527\\ \text{Oxide} = 19{\cdot}473\end{array}\right\} = 100$$

When boiled in alcohol, after being dried at 300° FAHR., this salt is not wholly dissolved. Of that which was left undissolved in one experiment,

12·17 grains left 3·09 oxide of lead = 25·31 per cent.
8·805 grains left 2·20 oxide of lead = 24·99 per cent.
in another experiment . . (15.)
8·57 grains left 2·232 oxide of lead = 26·04 per cent.
11·491 grains left 3·026 oxide of lead = 26·33 per cent.

These results agree with the formula $(C_{40}H_{31}O_4) + PbO$, which gives

$$\left.\begin{array}{l}\text{Resin} = 73{\cdot}388\\ \text{Oxide} = 26{\cdot}612\end{array}\right\} = 100$$

The boiling alcoholic solution deposits on cooling a white precipitate. After being dried at 300°, the portions of this precipitate obtained from the above two experiments were burned in the air.

A. 5·21 grains left 0·994 oxide of lead = 19·079 per cent.
5·52 grains left 1·044 oxide of lead = 18·913 per cent. . . (16.)
B. 9·217 grains left 1·683 oxide of lead = 18·249 per cent.

The salt thus precipitated is therefore the same as that obtained by the direct action of the solutions of resin A. and of acetate of lead on each other. When boiled on this sesquisalt, therefore, the alcohol decomposes it, dissolving a *bi*salt and leaving a *neutral* salt insoluble, while from the solution of the former a *sesqui*salt precipitates on cooling. This accounts for a slight deficiency in the oxide of lead found in these salts; the presence of a small quantity of a more acid salt in each causing a higher percentage of resin than is given by calculation.

When to the acid solution containing acetate of lead and resin A. from which this *sesqui*salt has been separated by filtration, pure ammonia is cautiously added with repeated agitation; a further white precipitate falls, which is *probably* the neutral salt above described. I did not dry this salt, but transferred it whilst still moist into boiling alcohol. After repeated digestion a comparatively small portion only was dissolved, the boiling solution depositing the *sesqui*salt as it cooled. The insoluble salt was collected and dried at 300° Fahr.

5·93 grains left 2·452 of oxide of lead = 41·357 per cent.

It is therefore a *di*salt = $(C_{40} H_{31} O_4) + 2 Pb O$, which consists of

	By experiment.	Calculated.	
Resin =	58·643	57·96	(17.)
Oxide =	41·357	42·04	
	100	100	

It appears, therefore, that resin A. of mastic forms with oxide of lead at least four compounds.

a. $2 (C_{40} H_{31} O_4) + Pb O$ obtained in solution on boiling *b* in alcohol.

b. $3 (C_{40} H_{31} O_4) + 2 Pb O$ by mixing the alcoholic solutions of the resin and of acetate of lead, and by subsidence from boiling solutions of *a*.

c. $(C_{40} H_{31} O_4) + Pb O$ left insoluble when *b* is boiled in alcohol.

d. $(C_{40} H_{31} O_4) + 2 Pb O$ left insoluble when the fresh precipitate from ammoniacal solutions of the resin and of acetate of lead is boiled in alcohol.

The compound *a*, as will appear from the preceding detail, is hypothetical, but its existence is necessary to explain the action of alcohol on the *sesqui*salt, and is rendered almost certain by the existence of an analogous silver salt*.

B. *Salts of Silver*. When an *ammoniacal* solution of nitrate of silver in alcohol is poured into an alcoholic solution of mastic resin A, a pure white precipitate falls, soluble in large excess of ammonia, and becoming reddish brown in the sun's rays. Collected on the filter, and afterwards boiled with alcohol, it is nearly all dissolved, giving a slightly coloured solution, from which on cooling, a bulky white precipitate again falls. This bulky precipitate was dried by pressure between folds of bibulous paper, and afterwards at a temperature of 250° Fahr.

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* If from the solution of the *bi*salt a *sesqui*salt precipitate spontaneously, a more acid salt still must remain in solution. Three equivalents of a *bi*salt would allow two of a *sesqui*salt to fall, while one of a *ter*salt would remain in solution. I shall have occasion hereafter to mention other circumstances which render probable the existence of resinous salts which contain three equivalents of acid, and others which contain three of base.

Dried at 250°, and still in the state of a white powder, it left 15·788 per cent. of metallic silver when burned in the air.

At 300°, when it cohered, became of a dark colour, and gave a brownish red powder; it left 16·042 per cent. of silver. At 350° Fahr. when it began to fuse it left 15·844 per cent.

The mean of these three results is 15·891 per cent. of silver, or 17·066 of oxide of silver, therefore the salt consists of

$$\left.\begin{array}{l} \text{Resin} \;\; . \;\; . \;\; . = \;\; 82{\cdot}934 \\ \text{Oxide of silver} = \;\; \underline{17{\cdot}066} \end{array}\right\} \;\; . \;\; . \;\; . \;\; . \;\; . \;\; . \;\; . \;\; . \;\; . \;\; . \qquad (18.)$$

$$100$$

A compound represented by the formula $2\ (C_{40}\ H_{31}\ O_4) + Ag\ O$ should contain 15·88 per cent. of oxide of silver, from which it would appear that the above is a *bi*salt with a small excess of silver. That there are other salts of silver containing less acid is rendered probable by the difficulty of obtaining this *bi*salt without excess of base.

VI. *Constitution of the Resin Salts.*—There remains still one important question to be solved in regard to the constitution of the salts of this and the other resins. Does the resin unite with the oxide as a whole and without change, as muriatic acid unites with ammonia to form sal-ammoniac, or is it altered in any way, as the organic acids generally are when they combine with bases? Does it part with any of the oxygen or hydrogen it contains, when it forms a metallic salt? This inquiry is a very interesting one, and it is not at all answered by the experiments above detailed. The results obtained are sufficiently wide of the quantities indicated by theory, to admit of considerable changes in the constitution of the resin, the existence of which can only be discovered by the ultimate analysis of the several compounds. In the paper of Rose on the constitution of colophony and some of the other resins*, the relative quantities of acids and base in several of the salts is investigated, but no ultimate analysis of them is given. He only says, that, by the ultimate analysis of the salts, he obtained nearly the same composition for the resin as by analysing the pure resin itself. From this it is fair to infer, that he found a slight difference, which he would naturally attribute to some impurity or imperfection in the salt employed, and would not, therefore, consider these results to be deserving of equal confidence with those obtained from the pure resin. In the actual state of our knowledge in regard to the theory of the saline compounds of organic substances possessing acid properties, it would be a great step in advance were the constitution of the resinous salts accurately made out, and it might be expected to throw much light on the relative constitution of the different resins themselves.

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* Poggendorff's Annalen, xxxiii. p. 32.

1. Of the neutral salt of lead above described, 11·9 grains gave $\ddot{C} = 25{\cdot}14$, and $\dot{H} = 7{\cdot}372$ grains. This is equal to

	(19.)	$PbO + (C_{40}H_{31}O_4)$ gives
Carbon . . =	58·416	58·361
Hydrogen . =	7·372	7·384
Oxygen . . =	7·882	7·643
Oxide of lead =	26·333	26·612
	100	100

2. Of the sesquisalt of lead $2\,PbO + 3\,(C_{40}H_{31}O_4)$ 11·28 grains gave $\ddot{C} = 26{\cdot}732$, and $\dot{H} = 8{\cdot}446$ grains. This is equal to

	Experiment.	Calculated.	
Carbon . . =	65·529	64·044	. . . (20.)
Hydrogen . =	8·446	8·103	
Oxygen . . =	7·776	8·379	
Oxide of lead =	18·249	19·477	
	100	100	

The first of these results is very near the theoretical constitution, and could it be *exclusively* depended upon, would indicate $C_{40}H_{30}O_4$ as the formula for the resin. The second analysis exhibits an excess of carbon, but the salt was not pure, as the quantity of lead it contained indicates. Want of material prevented me from repeating the analysis. Both results, however, are equally satisfactory in showing, that, in combining with the oxide of lead, the resin parts with none of its oxygen, and that the base does not replace any of the elements which it contains in an uncombined state.

3. Of the *sub*salt $2\,PbO + (C_{40}H_{31}O_4)$ dried at 300° Fahr.

A. 13·6 grains gave $\ddot{C} = 22{\cdot}54$, and $\dot{H} = 7{\cdot}064$

B. 14·11 grains gave $\ddot{C} = 24{\cdot}00$, and $\dot{H} = 7{\cdot}127$

C. 8·925 grains gave $\ddot{C} = 14{\cdot}77$, and $\dot{H} = 4{\cdot}595$.

These are equal to

	A. (21.)	B. (22.)	C. (23.)
Carbon . . =	45·827	47·033	45·760
Hydrogen . =	5·767	5·612	5·720
Oxide of lead =	41·355	41·355	41·355
Oxygen . . =	7·049	5·998	7·165
	100	100	100

The formula requires

40 Carbon . . =	3057·48 =	46·09
31 Hydrogen . =	386·86 =	5·86
2 Oxide of lead =	2788·99 =	42·04
4 Oxygen . . =	400·00 =	6·01
	6633·34	100

The second analysis B. exhibits a considerable excess of carbon, probably from an error of analysis. All the three, however, agree in representing the hydrogen as less than the formula adopted for the resin requires. A salt consisting of ($C_{40}\ H_{30}\ O_4$) + 2 Pb O would contain

Carbon	. . =	46·16
Hydrogen	. =	5·65
Oxide of lead	=	42·12
Oxygen	. . =	6·07
		100

The quantity of hydrogen is even sufficiently small to allow us to represent the resin by ($C_{40}\ H_{29}\ O_4$), while the neutral salt appears to contain ($C_{40}\ H_{30}\ O_4$). We can draw no inference in regard to the amount of hydrogen it contains from the analysis of the sesquisalt, from its apparent impurity; but in the other salts of lead the hydrogen present is manifestly less than in the pure resin according to the analyses above given (4, 7 and 8.). Can the metal replace the hydrogen, giving in the one $C_{40}\ H_{30}\ Pb_1\ O_5$, and in the other $C_{40}\ H_{29}\ Pb_2\ O_6$, and can such be the general constitution of the resin salts? This can only be determined by numerous and refined analyses of these compounds.

4. The salt of silver prepared, as already described, by precipitation and subsequent resolution in alcohol, and containing 16·954 per cent. of oxide of silver, gives a different result from those of lead in regard to the constitution of the resin.

A. 12·565 grains after heating to 300° FAHR., when it began to fuse, gave $\ddot{C} = 30·74$, and $\dot{H} = 9·704$.

B. Heated to 250° only, 11·38 grains gave $\ddot{C} = 28·293$, and $\dot{H} = 8·9$ grains.

These results give per cent.

	A. (24.)	B. (25.)	Calculated.
Carbon . . . =	67·595	68·673	68·589
Hydrogen . . =	8·581	8·689	8·398
Oxygen . . . =	6·870	5·684	6·732
Oxide of silver =	16·954	16·954	16·281
	100	100	100

The third column is calculated according to the formula 2 ($C_{40}\ H_{30}\ O_3$) + Ag O, and approximates as closely to the experimental results as was to be expected from a salt containing an excess of oxide, probably from the presence in small quantity of a less acid compound.

It appears, therefore, that in uniting with oxide of silver, the elements of an atom of water have been given off, the resin $C_{40}\ H_{31}\ O_4$ becoming $C_{40}\ H_{30}\ O_3$ + H O. And yet that this is not owing to a simple replacement of hydrogen by silver in the salt, is shown by its containing two equivalents of resin to one of oxide, 2 H O being given off while Ag O only is taken up. It is by no means clearly deducible from these analyses, therefore, that the constitution of the resin of mastic A. is truly represented by $C_{40} H_{30} O_3 + H O$, since during the treatment with silver and ammonia some deeper change may have been effected, giving rise to a new resin represented by $C_{40} H_{30} O_3$.

And that this is really the case, seems to be confirmed by an examination of the ammoniacal alcoholic solution from which the salt of silver has been precipitated. When this solution is decanted, and by distillation is concentrated and deprived of its excess of ammonia, it becomes of a brown colour, and a dark brown resin falls to the bottom of the retort or of a jar, into which the solution may be poured. On further concentration, after cooling, more of this resin falls, after which a precipitate of a white silver salt makes its appearance.

Separated by decantation, and kept for twelve hours at 212° Fahr., at which temperature it melted readily,

A. 5·089 grains of this brown resin burned in the open air, left 0·85 of silver, equal to 1·786 per cent. of oxide.

B. 10·97 grains burned with oxide of copper, gave $\ddot{C} = 31·67$, and $\dot{H} = 10·565$.

Correcting B by A, the resin consists of

	(26.)	$C_{40} H_{32} O_3$ gives
Carbon	= 81·280	81·385
Hydrogen	= 10·895	10·620
Oxygen	= 7·852	7·995
	100	100

When dissolved in alcohol a small quantity of a dark brown silver salt was separated. After filtration, evaporation, and fusing at 212° for several hours, the resin was obtained nearly free from silver.

A. 3·061 grains left, when burned in the air, 0·016 = 0·522 per cent.

B. 9·158 grains gave $\ddot{C} = 26·525$, and $\dot{H} = 9·0$.

C. After heating for several hours at 300° Fahr., during which a fragrant odour was emitted, 10·155 grains gave $\ddot{C} = 29·40$, and $\dot{H} = 9·933$.

These gave for the constitution of the resin

	A (27.)	B (28.)
Carbon	= 80·510	80·474
Hydrogen	= 10·976	10·925
Oxygen	= 8·514	8·601
	100	100

If we admit, as is very probable, that the excess of hydrogen and oxygen indicated by these analyses over those obtained in analysis 26. is due to the presence of a small quantity of water, even after heating at 300° Fahr., the three results become entirely accordant*, and leave little doubt that the constitution of the resin is expressed at least very approximately by $C_{40}\,H_{32}\,O_3$. In looking back upon our previous analyses also, we find in No. 6. a very close agreement with these now given; from which, compared with No. 11., it would appear that by the long continued action of a temperature not below 350° FAHR. the resin of mastic A, besides other alterations, is changed into a soluble resin containing *three*, and into an insoluble resin containing probably *five* equivalents of oxygen.

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* The discordance in the results thus obtained for the same mass, of the same resin before and after solution

The constitution of the resin formed during the preparation of the silver salt, seems thus to confirm the opinion above stated, that a deeper change is produced upon resin A during the treatment with nitrate of silver and ammonia, than the simple extrication of the elements of an atom of water. The large quantity of hydrogen present in the new resin especially supports this view. In the silver salt we have taken $C_{40}\,H_{30}\,O_3$ as the most probable expression for the resin it contains, assuming, as is usually done, that the lowest amount of hydrogen obtained by analysis is greater than the truth. In the analysis of the resin itself the same precautions were taken, yet $C_{40}\,H_{32}\,O_3$ contains the lowest amount of hydrogen which the results permit us to adopt. The presence of moisture in the oxide of copper could not, with the precautions adopted, occasion so great an excess of hydrogen; and in analysis (26.) there is no corresponding excess of oxygen to justify us in attributing the difference to hygrometric water in the resin itself.

5. With the view of investigating more closely the action of oxide of silver on the resin A, I dissolved a portion of it in alcohol, added to the solution a little caustic ammonia, and afterwards an alcoholic solution of nitrate of silver. The white salt which fell was collected on the filter, washed with cold alcohol, and afterwards dried at 300°. It melts at a low temperature in the alcohol it retains, and is obtained in the form of a dark red porous mass. By a careful regulation of the heat, however, it may be dried without undergoing fusion.

5·177 grains, burned in the air, gave 0·923 of metallic silver = 17·829 per cent.

4·404 grains gave 0·79 = 17·94 per cent. of silver, or 19·26 of oxide of silver.

11·632 grains gave with oxide of copper $\ddot{C} = 26{\cdot}87$, and $\dot{H} = 8{\cdot}69$.

These results give for the composition of the salt

	(29.)	In equivalents.
Carbon	= 63·87	= 40
Hydrogen	= 8·30	= 31·8
Oxygen	= 8·57	= 4·1
Oxide of silver	= 19·26	= 0·6
	100	

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in alcohol, is not without interest in a research into the general constitution of the resins. It shows that, with all our precautions, small differences will occur from circumstances not yet understood, and that in our selection of a formula to represent the constitution of a given resin, we ought to allow *analogy* to influence us in some degree where the results of analysis are not sufficiently numerous and concordant unequivocally to settle the question. To this point I shall have occasion to revert in a future paper on the constitution of certain other resins.

The resin in this salt is evidently $C_{40} H_{31} O_4$, or in combining with the silver by this method of preparation it undergoes no change. There is present a considerable excess (one-sixth of the whole) of silver, probably from an admixture of a less acid salt.

After the separation of the salt thrown down as above, nitrate of silver was still added as long as a precipitate fell. This second precipitate was collected and digested in boiling alcohol. A transparent slightly brownish solution was obtained, from which on cooling a white compound fell, but at the same time a considerable portion of a dark brown resin or resinous salt was precipitated on the sides and bottom of the retort in which the solution was effected. The white precipitate was collected and dried at 300° Fahr.

4·167 grains left 0·64 of metallic silver = 15·36 per cent., or 16·496 of oxide of silver.

A. 11·975 grains, burned with oxide of copper, gave $\ddot{C} = 28{\cdot}62$, and $\dot{H} = 9{\cdot}195$.

B. 8·67 grains gave $\dot{H} = 6{\cdot}703$, the carbonic acid being lost.

The salt therefore consists of

	A. (30.)	B.	$2 (C_{40} H_{31} O_4) + Ag O$ gives
Carbon	= 66·086		66·79
Hydrogen	= 8·530	8·59	8·46
Oxide of silver	= 16·496		15·88
Oxygen	= 8·888		8·87
	100		100

The bisalt of oxide of silver therefore, prepared without large excess of ammonia, is not decomposed by solution in pure alcohol, the change produced upon it and upon the resin in the former experiment being due either to the excess of ammonia or to the long boiling. Still though the salt obtained in this instance contained the unchanged resin A, yet when the alcoholic solution from which the precipitates were originally thrown down, was concentrated by distillation, there fell, on allowing the solution to cool, first, a portion of a white precipitate, and afterwards, on still further concentration, a quantity of a dark coloured resin, closely resembling the $C_{40} H_{32} O_3$, above described and analysed. It appears therefore to be easily reproducible, though the circumstances *necessary* to its production require further investigation.

In regard to the hydrogen contained in this salt, the analysis seems to indicate thirty-one equivalents, which does not agree with the change undergone by the resin in combining with the oxide of lead. It is not impossible, however, that the true formula for the salt may be $Ag O + (C_{40} H_{30} O_4) + (C_{40} H_{31} O_4)$, which would give 8·34 per cent. of hydrogen, a quantity not differing from 8·53 found by analysis, more than the ordinary results of experiment do from the calculated results; but upon this subject I would not be understood to express a decided opinion until it has undergone further examination[*].

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[*] In all these analyses the method of Liebig has been adopted, with the use of the cork; there seems, however, much weight in the observation of Berzelius, that where the number of atoms of hydrogen is great this method may lead to error. While a damp cork may give out, a perfectly dry one may absorb moisture.

VII. *Insoluble Resin of Mastic* (Resin B.). When crude mastic resin is digested in cold alcohol a variable quantity remains undissolved. This portion is soft, tenacious, and elastic like bird-lime, of a silky lustre, and may be boiled in alcohol without sensible diminution. When boiled in water it becomes harder and less elastic; but when deprived of water by drying at 300° FAHR., at which temperature it is in a semifused state, it may be drawn out into long fibres, and even when cold it is tough and has some of the elasticity of caoutchouc.

1. Burned in the open air it left a small gray residue of carbonate of lime, being earthy matter mixed with the natural resin.

6·358 grains left 0·06 = 0·945 per cent.

2. Burned with oxide of copper after drying at 300° FAHR.

A. 12·863 grains gave $\ddot{C} = 38{\cdot}624$, and $\dot{H} = 12{\cdot}645$.

B. 11·605 grains gave $\ddot{C} = 34{\cdot}75$, and $\dot{H} = 11{\cdot}395$.

These results, allowing for the small quantity of foreign matter present (0·954 per cent.), are equivalent to

	(31.)	(32.)
Carbon =	83·823 =	83·589
Hydrogen =	11·027 =	11·013
Oxygen =	5·160 =	5·398
	100	100

The formula which approaches nearest to these results is $C_{40}\,H_{31}\,O_2$, giving

40 Carbon =	3057·480 =	83·613
31 Hydrogen =	399·347 =	10·920
2 Oxygen =	200·000 =	5·467
	3656·827	100

The slight excess of carbon in the first analysis is probably due to some accidental cause. It may have been from a greater admixture of foreign matter in the one portion than in the other, that the small difference has arisen.

When this resin is exposed to a higher temperature it swells, gives off the odour of naphtha (?), and the porous mass, when cold, is no longer soft but brittle. Analysed in this state it gave the formula ($C_{40}\,H_{31}\,O_3$), but I did not ascertain how much foreign matter this specimen contained, and the coincidence of the result with this formula may be accidental.

This resin does not sensibly dissolve in a solution of caustic potash, it is, therefore, most probably not an acid resin.

VIII. *Conclusions.*—In regard to the resin of mastic, it therefore appears, from the preceding examination,

1. That it consists of two resins represented respectively by the formulæ (analyses 4, 7, 8, and 31, 32).

The soluble, or resin A. by $C_{40}\ H_{31}\ O_4$ an acid resin.

The insoluble, or resin B. by $C_{40}\ H_{31}\ O_2$ not an acid.

2. That even when considerable care is taken, a series of analyses may be obtained which do not indicate the true constitution of a resin (see analyses 1, 2, 3.).

3. That when exposed to the prolonged action of a temperature exceeding 300° Fahr., resin A. of mastic, among other changes, seems to be partly altered into a resin containing three, and into one containing five of oxygen, C_{40} being constant (analyses 6, and 11.). These resins are probably represented by

$A = C_{40}\ H_{32}\ O_3$, very soluble in alcohol.
$B = C_{40}\ H_{30}\ O_5$, sparingly soluble in alcohol.

4. That it combines with bases forming four series of salts, which in the case of oxide of lead consist of (see analyses 14, 15, 16, 17.),

1. Two equivalents of resin and one of oxide.
2. Three equivalents of resin and two of oxide.
3. One equivalent of resin and one of oxide.
4. One equivalent of resin and two of oxide.[*]

5. That in combining with bases, resin A. does not part with any of its oxygen, but that *if any change take place* in its constitution it is in the hydrogen being replaced by an equivalent proportion of a metal (analyses 19, 21, 22, 23, 24, 25, and 30.). The salts of lead would on this view be represented by

1. $C_{40}\ H_{31}\ O_4 + C_{40}\ H_{30}\ Pb_1\ O_5$.
2. $C_{40}\ H_{31}\ O_4 + 2\ (C_{40}\ H_{30}\ Pb_1\ O_5)$.
3. $C_{40}\ H_{30}\ Pb_1\ O_5$.
4. $C_{40}\ H_{29}\ Pb_2\ O_6$.

I regret to be obliged to leave this interesting part of the subject in an undetermined state, but it will require many often repeated analyses to determine whether this law be generally obeyed by the resins in their combinations with bases.

6. That resin A. by boiling in contact with ammonia and nitrate of silver, or perhaps with nitrate of ammonia[†], is changed into a resin, represented, when uncombined, by $C_{40}\ H_{32}\ O_3$. This resin forms a *bi*salt with oxide of silver, in which there

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[*] A notation for the resins is very desirable. If the initial letter of the name be adopted, and *r* be put over it to denote resin, and *a*, *b*, be subjoined to distinguish the several resins contained in one natural product, a notation would be obtained easily intelligible, occupying little space, and which would interfere with no other chemical notation yet adopted, Thus the two resins of mastic would be represented by $\overset{ra}{M}$, $\overset{rb}{M}$, and the salts of lead formed by resin A. by $\dot{Pb}\ \overset{ra}{M}_2$, $2\ \dot{Pb} + 3\ \overset{ra}{M}$, $\dot{Pb}\ \overset{ra}{M}$, $2\ \dot{Pb} + \overset{ra}{M}$.

[†] Since the above was transmitted to the Royal Society I have tried the effect of boiling resin A. with nitrate of ammonia. Solutions of this salt and of resin A. were mixed and repeatedly distilled, with fresh additions of alcohol. The resin became darker in colour, and much more soluble in boiling than in cold alcohol. After

is also an apparent replacement of hydrogen by silver*, the resin in this salt (analyses 24 and 25) being represented by $C_{40}\ H_{30}\ O_3$.

II. *Resin of Dragon's Blood.*

Two varieties of dragon's blood are common in this country; one in long stalks or quills inclosed in a leaf, or reed, and presenting the appearance of having been softened by heat previous to being rolled into this form; the other in lumps of considerable size, less compact, and mixed with a large quantity of vegetable and other foreign matter.

1. The purer variety in quills gave, on digestion in alcohol, and evaporation of the solution on the water bath, a resin almost black by reflected light, translucent, in thin fragments, and of a bright red by transmitted light, and in powder of a dark red colour. In preparing these resins it is difficult to determine at what temperature the whole of the alcohol or ether is driven off, and when the frothing up is due to the decomposition or disengagement of volatile matter from the resin itself. This is especially the case in regard to such as, like the present, melt at a low temperature. If the solution in alcohol or ether be evaporated at 180° Fahr. till it flows smooth and ceases to change in consistency, it will be found still to froth up and give off vapours having a peculiar, at first agreeable, afterwards *astringent* (?)† odour, if the heat be raised to 212°. That a change of composition ensues from a greater increase of temperature appears from the following analyses.

A. A portion of the solution in alcohol was evaporated and heated for a short time at 212° Fahr. 11·3 grains gave $\ddot{C} = 29{\cdot}43$, and $\dot{H} = 6{\cdot}084$ grains.

B. A second portion was evaporated at 212°, and afterwards raised for a few minutes to 220° Fahr. 11·16 grains gave $\ddot{C} = 29{\cdot}04$, and $\dot{H} = 6{\cdot}135$ grains.

C. The same heated to 280° Fahr. frothed up, emitting vapours having an *astringent* alcoholic odour, accompanied with considerable pungency. It was still entirely soluble in alcohol.

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boiling in water and drying at 300° Fahr., 10·04 grains gave $\ddot{C} = 28{\cdot}89$, and $\dot{H} = 9{\cdot}445$ grains, or per cent.

$$\left.\begin{array}{lcr} \text{Carbon} & = & 79{\cdot}56 \\ \text{Hydrogen} & = & 10{\cdot}45 \\ \text{Oxygen} & = & 9{\cdot}98 \end{array}\right\} 100.$$

By comparing this result with analyses 4, 7, and 8, it will be seen that the resin had undergone no change in composition, but was still $C_{40}\ H_{31}\ O_4$.

* If the resin be $C_{40}\ H_{31}\ O_3$, then $(C_{40}\ H_{31}\ O_3) + (C_{40}\ H_{30}\ O_3) + Ag\ O$ would agree satisfactorily enough with analyses 24 and 25.

† I venture to use this term, as it, more nearly than any correct one, describes the sensation.

12·42 grains gave $\ddot{C}$ = 32·965, and $\dot{H}$ = 6·94 grains.
These results are equivalent to

	At 212°.	At 220°.	At 280°.
Carbon	= 72·015	71·887	73·435
Hydrogen	= 5·982	6·108	6·208
Oxygen	= 22·003	22·005	20·357
	100	100	100

Had the results contained in the first and second columns been obtained from the same specimen, we might have concluded that up to 220° Fahr. no sensible decomposition takes place. As however these results, though they approximate to the formula C_{40} H_{20} O_9, are yet not *accurately* represented by any formula in which the carbon is expressed by C_{40}, I thought it necessary to analyse the resin prepared by evaporating a solution at a temperature never exceeding 180° Fahr.

2. For this purpose I took the common dragon's blood in lumps, and digested two separate portions in alcohol and ether, and filtered and evaporated the solutions in broad shallow dishes. The resin thus obtained was kept for twelve hours in a stove, the temperature of which was generally considerably under 180° Fahr. At this temperature the resin from the ethereal solution was to the last softer, and contained more air-bubbles than that from the alcohol. Both were of a brilliant red colour, contracted and cracked in every direction on cooling, and in fragments were exceeding electrical.

A. Of that from alcohol 10·25 grains gave $\ddot{C}$ = 27·523, and $\dot{H}$ = 5·95 grains.

B. Of that from ether 11·65 grains gave $\ddot{C}$ = 31·177, and $\dot{H}$ = 6·987 grains.

These are equal to

	A.	B.
Carbon	= 74·247	= 73·998
Hydrogen	= 6·450	= 6·663
Oxygen	= 19·303	= 19·339
	100	100

These results agree very nearly with the formula C_{40} H_{21} O_8, which gives

40 Carbon = 3057·480 = 74·218
21 Hydrogen = 262·091 = 6·362
8 Oxygen = 800·000 = 19·420
4119·571 100

3. The close accordance of these results with a formula containing eight of oxygen, induced me to return to the *reed* dragon's blood, with the view of ascertaining whether the two varieties were really unlike in constitution, or whether the differences obtained by analysis might not result from the mode in which the resin was extracted,

or the greater or less quantity of the solvent (alcohol or ether) they were permitted to retain. A portion of it therefore was digested in ether, and the solution evaporated.

A. Evaporated and kept for twelve hours at 150° Fahr., 12·31 grains gave $\ddot{C} = 32·606$, and $\dot{H} = 6·757$.

B. Kept for twelve hours at 190° Fahr., 11·12 grains gave $\ddot{C} = 29·563$, and $\dot{H} = 6·195$.

C. Of another portion evaporated at 212°, and kept at this temperature for six hours, 11·97 grains gave $\ddot{C} = 31·685$, and $\dot{H} = 6·44$.

These results are equivalent to

	A.	B.	C.
Carbon	= 73·240	= 73·512	= 73·193
Hydrogen	= 6·099	= 6·190	= 5·978
Oxygen	= 20·661	= 20·298	= 20·829
	100	100	100

By this prolonged heating of the resin obtained from the pipe or *reed* dragon's blood (as it is called in commerce), by means of ether, it is made to approximate nearer in constitution to the resin from the lump dragon's blood than that which was first analysed. Still there is a deficiency of nearly one per cent. in the carbon, and a considerable deficiency also in the hydrogen, compared with what is required by the formula $C_{40}\ H_{21}\ O_8$ as above given, and which is indicated by the analysis of the lump resin.

What is the cause of the difference, then, and which of the series of analyses is to be considered as representing the true constitution of the pure resin? I place most confidence in the series (2.), for these reasons: *first*, the lump resin is mixed with seeds and fragments of vegetable matter, and is therefore most probably the resin as it exudes from the tree: *second*, the results of the analysis of this variety give an exact formula analogous to those by which so many other resins are represented, and of which the constant C_{40} is a member; and *third*, the reed dragon's blood, from its porosity, its freedom from foreign substances, and its varying colour (from a bright red to a dirty brown), is evidently a *manufactured* article, the constitution of which therefore is not to be depended upon. Since I have been led to this conclusion, I have learned that a third variety of dragon's blood is brought to the market under the name of *strained*. Now to be strained the natural resin must be made very fluid, and can hardly fail to be more or less decomposed by the temperature necessary to produce the complete fusion. The *reed* variety is probably only the *strained*, made while soft into small rolls and wrapped up.

Conclusions.—The following therefore seem to be the conclusions to which the above experiments lead.

1. That the lump dragon's blood is the natural and pure resin, the strained and reed varieties being manufactured articles, and more or less decomposed.

2. That this resin retains alcohol and ether, as most other resins do, with considerable obstinacy, but that these solvents may be entirely expelled by a long-continued (ten or twelve hours) exposure to a temperature not higher than 200° Fahr.

3. That when thus perfectly dried it is represented experimentally by the formula $C_{40} H_{21} O_8$.

The two former of these results are of considerable practical importance in an inquiry into the constitution of the resins. Several of those resins which are of the greatest value in a commercial point of view, are said to be subjected to processes of manufacture, by the native collectors, before they are brought into the market; it is therefore of consequence to ascertain that the substance is procured in its unaltered state before it is subjected to analysis. I shall have occasion, in a subsequent part of this inquiry, to mention several resins, the alteration of which by manufacture or adulteration has caused me much loss of time, and, in some cases, prevented me from obtaining a formula on which much reliance is to be placed.

In regard again to the total expulsion of the alcohol or ether, so necessary as solvents of this class of substances, if we evaporate the resins in *mass*, and heat till all the alcohol is driven off, we may often produce decomposition before our purpose is accomplished. It is therefore of great importance to have learned, that long exposure of a thin film of the resin to a low temperature will fully deprive it of those volatile liquids.

Durham, March 12, 1839.

X. *On the Male Organs of some of the Cartilaginous Fishes. By* JOHN DAVY, *M.D. F.R.S., Assistant Inspector of Army Hospitals.*

Received December 27, 1838,—Read March 7, 1839.

IN a paper on the Torpedo, which was published in the Philosophical Transactions for 1834, I have briefly described the male generative organs of this fish as consisting of two firm oval testes, of vasa deferentia without vesiculæ seminales, and of a papilla opening into the cloaca, the common termination of the seminal and urinary passages.

Referring to Dr. MÜLLER's able work, "De glandularum secernentium structura penitiori," in which he treats of the testes of the Rays and Sharks, I find that his descriptions and views of these organs are not in accordance with the above. His words are, "Maxime singularis est genitalium masculorum in Rajis et Squalis conformatio; sunt enim organa glandulosa duplicis generis, altera, quæ hucusque tanquam testiculi descripta sunt, ex globulis, non vero ex ductibus seminalibus conflata, altera, plerumque pro epididymidibus habita, ex canalibus serpentinis composita, sed minime cum testiculis globulosis conjuncta; quare non epididymides sed glandulas proprias esse conjicio[*]."

He adds, "Organum alterum recte ab Ill. CUVIERO jam descriptum est. Dicit enim CUVIERUS, 'Ils sont grands, alongés, quoique larges et plats et s'étendent sous l'épine au-dessus du canal intestinal et de l'estomac. Leur plus grande partie est une agglomération de tubercles de la grosseur d'un pois, pressés les uns contre les autres, et présentant chacun un petite enfoncement au milieu de leur face externe. Ils tiennent ensemble par des filaments très-forts, et par la membrane extrèmement délicate, qui les enveloppe, et ne paroissent composés que d'un grand nombre de petits grains ronds très fins. L'autre partie de ces testicules singuliers est formée d'une substance glanduleuse homogène, qui en occupe en arrière, la portion la plus mince, et s'étend sous toute la face inférieure de la portion tuberculeuse[†].'"

After alluding to the similar observations of TREVIRANUS on the same organs in the *Squalus acanthias*, and mentioning in confirmation those which he himself had made on the Ray, he comes to the conclusion that the bodies which hitherto in these fishes had been called epididymides, having no connexion with the testes, at least yet discovered, are glands of a peculiar kind; and he conjectures, (stating, however, that it requires confirmation,) that the globular organs are the true testes, and that the spermatic fluid secreted by them, as in the instance of the Petromyzon and the Eel, instead of flowing through an appropriate canal, may burst into the cavity of the abdomen, and be discharged by the abdominal apertures[*].

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[*] Opus cit., p. 105.

[†] Leçons d'Anat. Comp. tom. v. p. 27.

Such high authority necessarily led me to doubt the correctness of my own observations, and to form the wish to repeat and extend them. This I have been able to accomplish; and I now propose to myself the honour of submitting the results to the Royal Society.

1. *Of the Male Organs of the Torpedo.*

Two good specimens of Torpedo, from the Mediterranean, of middle size, one an Oculata (*T. oculata*), the other a Tremola (*T. diversicolor*), belonging to the Museum of Natural History at Fort Pitt, have enabled me to institute a careful examination of the organs in question of these fishes. The result has accorded perfectly with my first observations made at Malta on the same species in the fresh state.

The testes appeared as I have briefly described them, both in relation to form, situation, and consistence. They exhibited indistinctly the globular structure described by Baron Cuvier in the passage quoted. In neither instance could I discover any traces of the supplementary or attached soft glandular structure noticed by Cuvier. The epididymides [the parts commonly so called] were comparatively large and distinctly tubular, terminating inferiorly in large tortuous vasa deferentia, which proceeded to the papilla or rudimentary penis within the verge of the anus, and superiorly were connected with the testes by a small number of vasa efferentia, applying the term hypothetically.

To this connexion, of course, my attention was particularly directed. The appearances were very satisfactory; small tubes or ducts could be clearly seen, passing from one to the other, and entering into the body of each. They were made perfectly apparent by means of minute dissection under water, and the immersion of the organs in dilute sulphurous acid, which has a property extremely useful in researches on minute structure, of imparting transparency to cellular tissue and serous membranes, and of expanding at the same time tubular and vascular parts.

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[*] The following is the passage at length. "Jam vero nunc evictum credo, epididymidem sic dictam, cujus conjunctionem cum testiculis nemo hactenus vidit, omnes potius supposuere, ne vero glandulam proprii generis esse. Quæritur nunc, utrum organorum testiculus sit, ac, si semen in globuloso testiculo paratur, quomodo semen excernatur. Posset aliquis seminis secretionem in organo altero ex canalibus composito ponere. Sed organon globulosum tam peculiare magnumque est, ut cum alio quopiam organo comparari nequeat. Tum vero meminimus structuram in Anguillis et Petromyzonibus testiculorum globulosam, in quibus semen non per ductum proprium evehitur, sed, uti ova in abdomen defluens, per orificium simplex excernitur. Quæro num etiam in Rajis et Squalis semen ex globulis in cavum abdominis propullulet et orificiis illis evehatur, quæ tam in masculis quam feminis Squalis et Rajis obveniunt? licet in feminis ova proprio duplici oviductu evehantur. Hoc observationes ulteriores evincere debent. Incertum etiam manet, cujus naturæ sit secretio alterius permagnæ glandulæ, utrum maximum momentum in hocce potius organo positum sit, an glandula testiculis succenturiata sit. Certe liquor glandulæ copiosissimus alius longe naturæ est ac testiculorum globulosorum et a glandula illa ipsa secernitur." He adds, "Itaque vera epididymis in piscibus non adest."—Op. cit. p. 107.

2. *Of the Male Organs of the Thornback* (*Raia clavata*).

On the 12th of October I had an opportunity, under very favourable circumstances, of examining a male Thornback, of large size, shortly after being caught. This being the breeding season of this Ray, its generative organs were fully developed, and every part of them was peculiarly distinct and large,—as the globular organs, considered by Professor Müller as testes; their soft milt-like appendage, attached to their inferior extremity, and partially bordering their inner margin; the massive epididymides, conjectured by Professor Müller to be glands of a peculiar kind; and the vasa deferentia, tortuous, of capacious dimensions, terminating in a kind of urethra, close to the necks of the two sacs, which I believe perform the double function of urinary bladders and of vesiculæ seminales.

The structure of the milt-like appendages, of the globular testes, and of the epididymides, was in accordance with Baron Cuvier's and Professor Müller's description of them. In neither of the two former could I observe any appearance of tubular structure, which was very strongly marked in the last.

As in the instance of the Torpedo, I made careful search after a connexion between the testes and epididymides; and using nearly the same means, I was able to satisfy myself that such a connexion exists, and in the same situation, namely, between the superior extremities of the two parts, where the space separating them is inconsiderable. The tubes of connexion, however, were smaller, and more difficult of demonstration than the analogous ones of the Torpedo.

As the vasa deferentia were distended with a cream-like fluid, which had very much the appearance of the spermatic fluid, it appeared probable that some satisfactory evidence might be obtained by instituting an examination of the contents of the different parts; and that it would be best effected by means of the microscope. The instrument I used was one of Mr. Ross's construction, provided with an achromatic object glass of one-eighth of an inch focal distance.

First, the fluid contained in the sacs, which I suppose to perform the double function of urinary bladders and of vesiculæ seminales, was submitted to examination. It was of the appearance and nearly of the consistence of cream. Under the microscope it was found to abound in animalcules in active motion, mixed with globules of different sizes. They were best seen when the fluid was diluted with a solution of common salt. The animalcules were proportionally of great length, not unlike portions of fine hair; one extremity was of extreme fineness, and seen with difficulty. Their motion was serpentine and vibratory, and of great velocity when most active, especially that of the tapering part; and their progressive motion was unquestionable, the effect of their own powers, independent of currents.

Next, the fluid from the vas deferens was examined, taken from its commencement, just after leaving the body of the epididymis. Its appearance to the naked eye, and its character under the microscope, were very similar to those of the preceding. It abounded in the same animalcules, also in active motion, many of them grouped together in bundles, and which so joined side by side acted together, the tapering part, in which the approximation was greatest, moving with great velocity in a vibratory manner.

Lastly, I examined the fluid yielded by the globular testis, procured by making an incision into its substance, and gently scraping the cut surface. The small quantity of fluid thus obtained was opaque, but not so thick as the last. Under the microscope it was found to contain animalcules similar to the preceding, and in motion, but less numerous, and not grouped, intermixed with small globular masses of an obscure granular and radiated structure.

3. *Of the Male Organs of the common Skate* (*Raia batis*).

On the 8th of November, under the same favourable circumstances as the preceding, I examined a male fish of the above species, in which the generative organs were fully developed, and as far as I could observe generally, were in no respects essentially different from those of the Thornback.

In this instance, for the sake of as much accuracy as possible, I began with an inspection of the fluids likely to throw light on the functions of the generative organs.

The fluid first subjected to the microscope was some contained in the cavity of the abdomen, transparent, with a small opaque sediment. It was found to contain globules of different sizes, the largest less than common pus-globules, and a few elliptical blood particles, without any animalcules.

The fluid next collected was that contained in the urinary bladders. It was nearly colourless and limpid. Under the microscope many small globules were visible in it, about one half the size of pus-globules, and a few animalcules, resembling those of the Thornback, and yet not precisely similar.

On pressing the vasa deferentia, where they pass on the inner side of each urinary bladder, a cream-like fluid was discharged into the cloaca through the papilla, the termination of the urinary, and, as I believe, seminal ducts. This fluid under the microscope was found to abound in animalcules, mixed with a few globules, both similar to those last mentioned; the former in active motion.

Next, the vas deferens was opened into before it passes behind the urinary bladder, and some fluid was obtained from it, not inconsiderable in quantity, and of the colour nearly and consistence of cream. Under the microscope it was found to abound in animalcules of a thread-like form, having one extremity excessively fine, very active in movement; their motion vibratory, as well as progressive; in every respect closely resembling those of the Thornback; and like them, owing to their vast number and being intermixed with many globules, to be seen distinctly the fluid required to be diluted.

The globular testis was next cut into; a portion of it was removed by a horizontal incision. It abounded in fluid, more liquid than that of the vas deferens, and less opaque. Under the microscope many animalcules were seen in it, precisely similar to those of the vas deferens, but not in motion, as if dead; and mixed with them were many globules of different sizes, the largest about the size of pus-globules, a few blood corpuscles, and some fragments of irregular form.

Lastly, an incision was made into the lower non-globular milt-like part, bordering the globular testis. It abounded in thick opaque fluid, of which a sufficient quantity was collected for examination by gentle pressure. Under the microscope it was found to contain a large number of globules of about the size and general appearance of pus globules, a few very much smaller, and a few animalcules, less distinctly formed than those in the globular portion and in the vas deferens, but clearly of the like kind.

The anatomical examination of the organs was next entered on. I have stated that at first view they appeared generally not to differ from those of the Thornback; a minute inspection confirmed this. A tubular connexion was found between the head of each testis and epididymis, not admitting of doubt; the tubuli were traced from the globular substance into the mass of the epididymis.

On the 24th of November another fish of this species was procured, in which also the male organs were in a very favourable state for examination, and which were examined with great care, having in view the doubtful points. No fluid in this instance was contained in the abdominal cavity; not a single drop could be collected.

As the globular and the milt-like testes, both of this Ray and of the Thornback, are connected with the epididymides by a delicate peritoneal covering, leaving a cavity on each side of the spine between the testis and epididymis, which descends close to the bladder, it occurred to me as possible, although not probable, that this cavity might be a channel between the respective testes and the cloaca. To endeavour to determine this, a small opening was made into the cavity, and it was forcibly distended with air by means of the blow-pipe, but no orifice inferiorly could be detected; the air was completely confined.

This done, the fluid contents of the different parts were subjected to microscopical examination in the following order; first, that of the urinary bladders; secondly, that of the milt-like testis; thirdly, that of the globular testis; and lastly, that of the vas deferens. The results in part were somewhat different. The fluid contained in the two urinary bladders amounted to four-fifths of a cubic inch; like the former it was colourless and transparent, with a very slight sediment. It contained a few globules, but no animalcules. The milt-like part of the testis yielded but little fluid; a minute quantity of it, collected by scraping gently an incised surface, exhibited under the microscope no animalcules, but many well-defined globular particles of different sizes, commonly smaller than pus-globules, and a few blood-corpuscles. The globular portion of the testis abounded in fluid of a creamy appearance, containing many animalcules mixed with globular particles; the former precisely similar to those observed in the first instance, and like them motionless. The fluid of the vasa deferentia, which flowed out on opening into them, amounted to seven-tenths of a cubic inch. It had the same character as that procured from those canals in the other specimen, and abounded in animalcules in every respect similar, and like them in active motion.

The anatomical examination, too, in this instance afforded a result precisely similar to the preceding; a connexion by means of tubuli was discovered between the head of each testis and epididymis, and nowhere else, after very careful search. The whole of the generative organs, with the urinary associated with them, were removed entire, and were immediately examined under spirit of wine.

4. *Of the Male Organs of Scyllium Edwardii.*

This is the only species of Shark, the male organs of which recently I have had an opportunity of examining; it was brought by Dr. ANDREW SMITH from the Cape of Good Hope; and to this gentleman I am indebted for permission to inspect it. The parts were not in the best condition; they had suffered from keeping, especially the testes. The epididymides were large; as were also the vasa deferentia, which terminated, as in the foregoing instances of the rays, in a kind of urethra, connecting the urinary bladders with the cloaca.

Having found that the spermatic animalcules of the Mammalia are but little liable to change, that they may be detected even in putrescent fluids, and may be kept for a long time in spirit of wine, I thought it worth while to examine with the microscope any fluid that might be found in the vasa deferentia of this Scyllium. A little turbid fluid mixed with grumous matter was procured by laying them open, in which, when diluted, animalcules were distinctly seen, resembling much those of the Rays.

This result induced me to try the contents of the testes and vasa deferentia of the Torpedo. The experiment was made with two fishes, varieties of the Tremola, rather below the middle size, which I had sent home from Malta, now more than four years ago preserved in spirits. The vasa deferentia of both were large; and when opened afforded pretty much opaque, white, semifluid matter. When diluted with water and placed under the microscope, animalcules were distinctly visible in it; thread-like, serpentine, finely tapering towards one extremity, very similar to those of the common Ray and Thornback, but decidedly smaller; and animalcules of precisely the same kind, but less numerous, were detected in the semifluid opaque matter obtained in minute quantity by scraping gently the cut surface of the testes. The animalcules in the testes and vasa deferentia of both specimens offered no perceptible difference.

5. *Of the Accessory Male Organs.*

Before entering on the inferences to be drawn from the foregoing observations relative to the functions of the different parts constituting the male organs which are contained within the abdominal cavity, I would wish to offer a few remarks on the external accessory organs, which have commonly been considered auxiliary to the more important internal ones.

They are the anal appendages, the characteristic of the male cartilaginous fishes, organs of complicated and curious structure, the use of which even at present is far from being understood.

The Torpedo, the common Ray, and the Thornback are the only species of Rays which I have yet carefully examined in relation to the organization of these parts. In each species they are very similar, consisting of articulated bones, muscles, mucous glands, mucous ducts, &c., and containing a large and remarkable gland associated with an elaborate and complicated structure. On account of the large size of the common Ray and its large anal appendages, and their full development, the gland and its accompaniments are seen in this fish to great advantage. In two specimens of *Raia batis* which I have examined, the gland was nearly the size of a chestnut, of a very elongated oval form, divided on one side, as it were, into two columns by a furrow or depression, in which were two rows of delicate projecting tubuli, the extremities of its excretory ducts. The substance of the gland was enveloped in a muscular coat, and this was covered with a vascular tissue. The gland itself was contained in a sac composed of three coats, an inner fibrous coat, a middle muscular, and an outer cellular one; and was surrounded with strong muscles, the principal flexor and extensor muscles of the organ*.

Moreover, at the inferior extremity of the sac, just below its outlet, there was a distinct cavity, formed of muscular walls and intersected by delicate tendinous fibres. In one instance, when under examination, the fish was still irritable, its muscles acting when stimulated, and then this part pulsated regularly and vigorously. It contained blood: I believe it to be an auxiliary heart, designed for circulating the blood in the appended organ. A similar structure exists in the same situation in the Thornback and Torpedo.

In the sac of the gland a cream-like secretion was found, and the same flowed out pretty copiously through the excretory ducts when pressure was applied to the gland. It was neither acid nor alkaline; it was slightly viscid; applied to the tongue no sensation was immediately produced, perhaps there was an after one approaching to acrid, but so slight as to be doubtful. Under the microscope it was found to abound in very minute, dense, spherical particles, twenty of which, at least, would be required to form a mass equal to a blood corpuscle of man. They had no appearance of independent vitality, and moved only when in currents.

The blood in the pulsating cavity, from which it is probable that the secretion just mentioned is formed, coagulated like ordinary blood on exposure to the air; but it was more dilute; and, what is remarkable, under the microscope its particles appeared to be smaller, and the majority of them not elliptical but globular.

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* Bloch in his description of these organs describes only two muscles; but there are more, some connecting the organ with the pelvis, others attached to the principal bones, and others to the smaller bones; which is, as might be expected, considering that eleven bones and one terminal cartilage enter into the composition of each organ. This number I found in the instance of the Thornback; Bloch states, that in this fish they are eight (Hist. Nat. des Poissons, iii. 672.).

How the anal appendages are constituted in Sharks I cannot speak from my own observations, having yet examined these organs only in one instance, that of *Scyllium Edwardii*, before referred to. From the descriptions of naturalists it may be inferred, that they vary more or less in organization in different genera; that in some, as probably in the genus Carcharias, there is a distinct gland, secreting an opaque fluid, similar to that of the Rays I have mentioned; but in others, as in the genus Scyllium, the gland is wanting, and its place is supplied by a sac, one for each organ, situated under the common integument of the lower part of the abdomen, communicating by a narrow elongated passage with the appendage*, and containing a slightly viscid fluid†, probably secreted by follicles situated between the fibrous inner coat and its outer muscular one. This structure I am informed by Dr. ANDREW SMITH, who it would appear first observed it, occurs in every species of the genus; and, as in one specimen, that of *Scyllium Edwardii*, the gland is not to be found (we carefully sought for it together), it is probably deficient in the others.

6. *Concluding Remarks.*

As in the Torpedo, the common Ray, and the Thornback, a tubular communication has been found to exist between the globular testes, and the bodies hitherto called epididymides; as the fluid in the former was found to be similar to that of those canals, continuations of the epididymides, which have commonly been considered as vasa deferentia; as this fluid, in its entozoa, possesses the essential character of a spermatic fluid; and, lastly, as it could not be detected free in the cavity of the abdomen, may it not be inferred, and is not the conclusion unavoidable, that the old opinion respecting the functions of these different parts is correct, and in accordance with the names which they have received?

The evidence on which this conclusion is founded is manifestly of two kinds; one anatomical, the other microscopical, connected with the constitution of the spermatic fluid. Preparations have been made of the parts showing the tubular connexion, which are deposited in the collection of comparative anatomy belonging to the museum at Fort Pitt.

Relative to the evidence derived from the nature of the spermatic fluid, I apprehend, now, it will generally be admitted as satisfactory. I have examined the spermatic fluid of many species of Mammalia, and the animalcules contained in it were in no instance more distinct than those of these fish; indeed in the latter they were perfectly so, and their character well marked. They were immediately killed by spirit of wine; they were torpid and motionless in a saturated solution of common salt; they became active when the solution was diluted; and in the unmixed fluid they retained life for three or four days after removal from the fish, until signs appeared of incipient putrefaction.

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* It communicates with the outer surface of the appendage by an opening close to the anal membrane, and also with the groove or tube of the organ, and thus with its internal surface. The one opening is not continuous with the other as in the Rays, a space equal to about a quarter of an inch intervenes.

† According to Dr. SMITH, the fluid contents are like very dilute white of egg.

It is true that Sir **Everard Home**, in the fifth volume of his Lectures on Comparative Anatomy, has denied that spermatic animalcules essentially belong to the prolific fluid, and was of opinion that they are a mere fiction of the mind, because neither he nor Mr. **Bauer** could detect them. His opinion at the present time, I apprehend, can have no weight, and it will be received only as a proof of the imperfection and feebleness of the instrument which he used, and serve as a warning, much as it is to be regretted, that no reliance is to be placed on his microscopical observations.

The testes of the Torpedo are distinguished from those of the Thornback and common Ray, by wanting that milt-like margin or appendage described by Baron **Cuvier**. Should it be found wanting in all the viviparous species of cartilaginous fishes, its presence in the oviparous may be considered as a link between them and the osseous oviparous fishes furnished with milts*. Perhaps in the milt-like part of the testes of the Ray and Thornback, the ova of the spermatic animalcules are developed; and, perhaps, in the globular portion they grow and are matured. The microscopical observations described seem to be rather in favour of this idea.

If the view which I have advocated of the functions of the testes and of the epididymides be received, the structure of the subordinate generative parts within the cavity of the abdomen, will, I believe, be found to be in perfect accordance, and to offer fresh evidence in favour of its correctness: I now allude to the vasa deferentia and the part or parts inferiorly connected with them.

The vasa deferentia in the three Rays which have been noticed are similar, and well adapted both to conduct and to hold in reserve such a fluid as the spermatic, for they are comparatively large and tortuous, and are provided with circular valvulæ conniventes, forming a vast number of cells. Both in the Ray and Thornback, and in the Torpedo, they terminate in what may be considered the urethra of a rudimentary penis, the end of which projects into the cloaca in the form of a papilla. In the Torpedo, which differs from the other two in having no urinary bladder, the spermatic and urinary ducts terminate near the mouth of the papilla, by separate and very minute orifices. In the Thornback and Ray, which are possessed of two urinary bladders, these ducts terminate near the fundus of the short urethra, and close to the neck of each bladder, or its opening into the common passage*. These bladders appear to be somewhat analogous to the bifid bladder of Frogs and Toads, the latter connecting them with the simple urinary receptacle of the higher vertebrata.

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* The spermatic fluid of the milt of the bony fishes bears some resemblance to the fluid of the milt-like portion of the testes of the Rays. I have examined it in the Herring, Smelt, Cod, Dab, and Pike; in the three first it appeared minutely globular, and no filamentous prolongations were distinctly visible in the animalcules; in the Pike a single filament was seen attached to many of the globules, and in the Dab, two. MM. **Dumas** and **Prevost** state that the spermatic animalcules of all the fishes they had examined are filamentous, and that the filament or prolongation had previously escaped observation on account of its great tenuity; an opinion I can readily adopt, as I could perceive the prolongations of the animalcules of the Dab and Pike only in a very favourable light, and by means of very nice adjustment. What the species of fishes were examined by these gentlemen is not noticed; their remark on the subject is made incidentally in their ingenious memoir on the spermatic animalcules of many of the vertebrata, with the promise to give the details on a future occasion (Ann. des Scien. Nat. tom. i. and ii.).

I have expressed the opinion that these organs may perform the double function of vesiculæ seminales and of urinary bladders, partly from their situation and connexion, and partly from their contents. I have mentioned, that in one instance, I found in them some spermatic animalcules, which may be considered equivalent to spermatic fluid. The result of a chemical examination of one specimen of the fluid which was contained in them, amounting nearly to a cubic inch, was favourable to its urinary character; for besides affording a little saline matter, principally common salt, it yielded a little animal matter very analogous to urea, soluble in alcohol, uniting with nitric acid, and the compound crystallizable and soluble.

The nature of the anal appendages and their functions, have from the earliest times of natural history been more or less a subject of controversy. ARISTOTLE considered them as characteristic of, and peculiar to, the male cartilaginous fishes. LORENZINI erroneously denied that they are distinctive marks of the sexes. WILLOUGHBY, RAY, ARTEDI and BROUSSONET considered them as organs of intromission, as penes. RONDELLET opposed this notion, and considered them as holders, in which opinion he was followed by BLOCH, who, I believe, was the first to describe them with tolerable accuracy and minuteness†. Recently, some naturalists appear to have adopted one conclusion, some the other. The majority favour the idea of BLOCH, that they are analogous, as he ingeniously endeavoured to prove, to arms or rather feet, and intended for seizure and holding fast. A small number, amongst whom M. DE BLAINVILLE is eminent, seem to have returned to the older notion that they are penes. BLOCH's argument against their being penes, is founded on their structure, in his opinion totally unfit for their supposed office. Those who maintain the opinion he opposes, lay stress on the appearance of the fluid secreted by the gland belonging to each appendage, so much resembling the spermatic fluid, and on the analogy in certain of the reptiles of a double oviduct and double penis.

Objections, it appears to me, are unavoidable to both these views. As regards the latter, it is highly improbable that there should be two sources of spermatic fluid: moreover, it has appeared that the fluid of the appendages is without the characteristic quality of the spermatic fluid, containing no entozoa, and seeming to be of a peculiar kind. As regards BLOCH's view, it seems improbable, were the appendages designed for prehension, that they would be furnished with a gland copiously secreting a lubricating fluid: nor does their general structure or situation appear to be apposite for the purpose he imagines, especially as he is of opinion that the rudimentary penis is applied in the act of impregnating to the surface of the cloaca, where the mouth of the oviducts or uterine cavities open.

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* Delicate probes passed through the bladder, ureter, and vas deferens of the common Ray and Thornback meet at the bottom of the short urethra, common to both sides; the openings into it are small and contracted; the vas deferens on each side terminates in it by a projecting papilla, having an oblique direction. I have carefully sought for a termination of these ducts into the bladder itself, but in vain. I mention this particularly, because a passage in the valuable Descriptive Catalogue of the Museum of the Royal College of Surgeons would seem to imply such a termination (vol. iv. p. 51. No. 2394.).

† Hist. Nat. des Poissons, iii. p. 672. His description was confined to these organs in the Thornback.

Reflecting on the subject and on the inadequacy of former hypotheses, it has occurred to me as possible, that these organs may be designed for intromission and retention like the penis of the Dog. On this idea, the abundant secretion of the gland, as a lubricating fluid, would be of manifest use, and the action of the different parts might admit of explanation. And, as the sacs connected with the appendages in certain of the Sharks open appropriately, they may be supposed to be designed for the same use[*]. In venturing to bring forward this conjecture, I beg to be understood, that I attach no kind of importance to it. I am fully aware that it is liable to objections. I shall be satisfied should it lead to further inquiry, by which alone the true use of these mysterious organs can be determined.

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[*] After writing the above, on referring to ARISTOTLE, that great and curious storehouse of natural history, I found that a similar idea had been entertained in his time, and that there were some who declared they had witnessed the fact. "Sunt qui se vidisse confirment nonnulla ex cartilagineis aversa modo canum terrestrium cohærere." De Hist. Animal. L. v. c. v.

Fort Pitt, Chatham,
Dec. 22, 1838.

XI. *Researches on the Tides.—Tenth Series. On the Laws of Low Water at the Port of Plymouth, and on the Permanency of Mean Water. By the Rev.* W. Whewell, *B.D. F.R.S., Fellow of Trinity College, Cambridge.*

Received April 11,—Read June 6, 1839.

In former communications to the Society, the laws of high water at Plymouth and other places have been the subject of my researches. These being obtained, the laws of low water are a subject of importance and interest on many accounts. The first ground of my pursuing this subject was the desire to ascertain how far the *mean water*, that is, the height midway between high and low water, is permanent during the changes which high and low water undergo. That it is approximately so at Plymouth, had been ascertained both by Mr. Walker and myself, by means of a comparison of a short series of observations. But it was desirable to know with more exactness what was the real amount of this permanency, when, by using a long series of observations of high and low water, the irregularities arising from accident, and from taking imperfect cycles of inequalities, were eliminated.

There was another reason which made this inquiry important at the present time. An operation has been recently carried on by the direction and at the expense of the British Association, with a view of ascertaining what surface ought to be taken as the permanent level of the sea. A Level Line has been carried with great care and accuracy from the north shore of Somerset to the south shore of Devon; and the position of this line has been fixed, so as to be recognised at any future time, by means of marks* at Axmouth, at East Quantockshead, at Stolford, and at Portishead. This line has also been referred to the sea at its extremities; and the observations show that the height of mean water coincides, at least very nearly, at different places, as well as at the same place at different times. While the difference of levels of low water at Axmouth on the English Channel, and Wick Rocks on the Bristol Channel, is not less than twelve feet; the mean water at those two places coincides in level within a few inches. In order to determine further what accuracy may be attained

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* These marks, and their respective heights above a certain arbitrary level, are as follows:

	Height.
Copper bolt in a granite block at Axmouth	83·6513 feet.
Copper bolt in a granite block at East Quantockshead........	244·4365 feet.
Copper bolt in a granite block at Stolford..................	125·1114 feet.
Iron bolt in the rock at Portishead........................	102·5795 feet.

The account of the operation of carrying the Level Line to these different points, and comparing its position with that of the surface of the sea, will appear in the Transactions of the British Association for 1838.

in this result, we are led to inquire what is the degree of permanency at one place. I may further add, that it cannot but be instructive to know how far the corrections of the height and time of low water, for lunar parallax and declination, agree in form and amount with the same corrections already obtained for high water.

I took, therefore, six years of observations at Plymouth (1833—1838), made, as I had reason to believe, with care and accuracy under the superintendence of Mr. WALKER, at present the Queen's Harbour Master at that port: and I had them discussed by Mr. ROSS of the Hydrographer's Office at the Admiralty; by which gentleman, on this as on former occasions, the requisite calculations have been performed with much zeal and intelligence.

The method employed in discussing the observations was the same, with slight modifications, as in former researches. The low waters were referred to a transit of the moon anterior by about two days to the time at which each occurred; and according to the hours of these transits, were divided into twelve horary groups, from 0^h to 1^h, from 1^h to 2^h, and so on. In order to find the laws of the heights, the mean height was taken for each of these groups. The mean parallax for each group was very nearly the absolute mean lunar parallax; and the mean declination for each group differed from the absolute mean by a small quantity, according to a known law. Hence the mean heights of the separate horary groups, compared with one another, gave the law of the height as depending upon the hour of transit; that is, they gave the semimenstrual inequality of height for low water.

1. *Of the Permanency of the Height of Mean Water.*

The height of low water, cleared of the effects of lunar parallax, and very nearly of the effects of lunar declination, and compared with the height of high water similarly cleared, enabled me to ascertain whether the mean water also was affected by a semimenstrual inequality. The following are the results of this calculation, keeping the six successive years separate.

Height of Mean Water at Plymouth.

Transit	h m 0 30		h m 1 30		h m 2 30		h m 3 30		h m 4 30		h m 5 30		h m 6 30		h m 7 30		h m 8 30		h m 9 30		h m 10 30		h m 11 30		Mean.	
	ft.	in.	ft.	in.	ft.	in.	ft.	in.	ft.	in.	ft.	in.	ft.	in.	ft.	in.	ft.	in.	ft.	in.	ft.	in.	ft.	in.	ft.	in.
1833.	9	10¾	10	0	10	1¾	10	3¾	10	5½	10	6½	10	6	10	4¾	10	3¾	10	1¾	9	11	9	10½	10	2·5
1834.	10	0	10	2	10	3	10	4½	10	6	10	7	10	6	10	2½	10	1	10	0½	9	11½	9	10½	10	2·6
1835.	10	1	10	1	10	1½	10	3½	10	4¼	10	6	10	5¼	10	3	10	1¾	10	0½	10	0	10	0¾	10	2·4
1836.	10	1½	10	2½	10	4	10	6½	10	7	10	8½	10	6½	10	4½	10	3½	10	1½	10	1½	10	0½	10	4·0
1837.	10	0	10	1	10	2	10	4	10	6½	10	6½	10	5	10	2½	10	1½	10	0	10	0	9	11	10	2·3
1838.	10	4	10	4½	10	6½	10	8	10	8½	10	9	10	8¾	10	5½	10	5½	10	4	10	4¼	10	4½	10	6·1

It appears from this table that the height of mean water is constant from year to year within two or three inches.

It appears also that the mean water for each fortnight has a semimenstrual inequality amounting to six or seven inches; the height of the mean water being greatest when the transit is at 6^h, and least when the transit is at 12^h. The immediate cause of this inequality of the mean water is, that the semimenstrual inequality of low water is greater than that of high water; as I shall soon have further occasion to remark.

How far this small semimenstrual inequality of the height of mean water is universal for all places, I am not at present able to pronounce. But I am strongly disposed to believe that the difference in the amount of the semimenstrual inequality of high water and of low water depends upon local circumstances; and therefore that the semimenstrual inequality of the mean height is a casual and partial result; the general rule being that the mean height is constant, except so far as it is slightly modified by local circumstances.

2. *Of the Semimenstrual Inequality of the Height of Low Water at Plymouth.*

The height of low water is affected by the moon's declination, and hence the mean height of low water for a year depends upon the mean declination. Now the mean declination for the year is different in successive years in consequence of the change of position of the moon's orbit. Hence the mean height of low water will be different in successive years. The same may be said of high water. The following is the comparison of the successive years now under discussion with reference to this circumstance.

Mean Annual Low Water and High Water at Plymouth, compared with the Mean Lunar Declination.

	1833.		1834.		1835.		1836.		1837.		1838.	
Mean declination .	14°	14′	15°	17′	16°	15′	17°	12′	17°	37′	17°	50′
	ft.	in.	ft.	in.	ft.	in.	ft.	in.	ft.	in.	ft.	in.
Low water	4	1	4	2	4	3¼	4	5¼	4	2½	4	7½
High water......	16	4	16	3	16	1½	16	2½	16	2	16	4¼

It appears from this, that in the mean low water there is a tolerably regular increase corresponding to the increase of declination, and amounting to about two inches for each degree of declination. In the high waters, this change is less marked. When we obtain a declination table from the observations, we find that about the middle part of this table, (from decl. 12° to 18°), the correction for declination is about one inch for each degree, which accordingly I shall adopt.

Hence the semimenstrual lines for successive years, obtained by merely taking the mean results of the year, will differ in consequence of the different mean declinations of the moon. And in order to obtain from them a table suited to the absolute mean of lunar declination, which for such purposes is about 16½°, we must correct the result of each year by a proper quantity; which quantity may, without sensible error, be supposed the same for all hours of transit, and at Plymouth will amount, as I have said, to about one inch for each degree.

If we suppose the moon to move in the ecliptic, the mean of all her simple declinations will be less than 15°; but since the declination correction varies as the square of the declination, we must take as the mean declination of the tables, that which gives the mean correction; which is about 16½°, as stated above. The mean declinations in page 153 are obtained by adding the simple declinations only. Hence it appears that the year which corresponds most nearly to the mean declination correction is the year 1833; and to this, therefore, the others will be reduced.

The semimenstrual inequality for each year will be given at the end of the paper: and for the reasons already stated, I subtract one inch from the heights for 1834, two inches for 1835, three inches for 1836, three inches for 1837, and four inches for 1838. I thus obtain the following results.

Semimenstrual Inequality of the Height of Low Water at Plymouth reduced to 1833.

Moon's Transit.	h m 0 30	h m 1 30	h m 2 30	h m 3 30	h m 4 30	h m 5 30	h m 6 30	h m 7 30	h m 8 30	h m 9 30	h m 10 30	h m 11 30	Mean.
	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.
1833.	2 1	2 7	3 6	4 8	5 9½	6 7	6 5	5 5½	4 3	3 2	2 4	2 0	
1834.	2 2	2 11	3 8	4 8	5 9½	6 7	6 5	5 3	4 1	3 2	2 5	2 0	
1835.	2 3	2 9	3 6	4 8	5 8½	6 7	6 4	5 4½	4 3½	3 2	2 6	2 1½	
1836.	2 2½	2 10	3 8½	4 11	6 0	6 10	6 6	5 5	4 3½	3 2	2 5½	2 1	
1837.*	2 0	2 7	3 5	4 7	5 10	6 6	6 3	5 2	4 0	2 11	2 4	2 0	
1838.	2 4½	2 11	3 10	4 11	5 11	6 8	6 7½	5 5	4 4	3 3	2 8½	2 5	
Mean..	2 2¼	2 9¼	3 7¼	4 9	5 10	6 7½	6 5	5 4½	4 2½	3 1½	2 5½	2 1¼	4 1½

In order to compare this with the semimenstrual inequality of high water at the same place, I take the mean of these heights, which I find to be 4 feet 1½ inch, and I express each height by its defect or excess with reference to this mean. In like manner, taking the table of the semimenstrual inequality of high water at Plymouth, I find the mean height to be 16 feet 3½ inches; and I express each other height by the excess or defect with reference to this. In this way I obtain two comparable expressions for the semimenstrual inequality of low and high water as follows:

Moon's transit	h m 0 30	h m 1 30	h m 2 30	h m 3 30	h m 4 30	h m 5 30	h m 6 30	h m 7 30	h m 8 30	h m 9 30	h m 10 30	h m 11 30
	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.
Low water....	−23¼	−16¼	− 6¼	+ 7½	+20½	+30	+27½	+15	+ 1	−12	−20	−24¼
High water....	+16½	+12½	+ 5½	− 4	−13½	−20½	−20	−12½	− 3	+ 8	+14	+17¼

Thus it appears that the *semimenstrual inequality of low water at Plymouth is greater than that of high water* in the ratio of 3 to 2 nearly, the total amounts being respectively 53½ inches, and 37 inches. The total semimenstrual inequality of the mean water is half the difference of those two, or eight inches nearly: but this is to be reduced in consequence of the correction for parallax.

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* There is a seeming anomaly in the results of 1837, which is caused, at least in part, by employing apparent time for that year, while the others (calculated afterwards) were referred to mean time.

3. *The Parallax Correction of the Height of Low Water at Plymouth.*

The parallax correction is obtained from all years alike, by taking the residue of each observation which remains when the semimenstrual inequality is taken away, and arranging these residues (for each hour of the moon's transit) according to the parallax. The mean declination for each column of such an arrangement is very nearly the absolute mean declination for the year; and hence the different heights will depend almost entirely upon the different parallaxes. In this manner we obtain the effect of parallax, arranged according to hours of moon's transit. But as the effect upon the height is nearly the same for all hours of transit, I take the mean of all the twelve hours, and thus obtain the parallax correction for the height of low water. I place along with this the parallax correction for the height of high water at the same place. Hence it appears that at Plymouth the parallax correction for height is somewhat greater for low water than for high water.

Parallax	54′½	55′½	56′½	57′½	58′½	59′½	60′½	61′½
Low water ..	+ 8	+ 6	+ 3½	+ 0½	− 4	− 8½	−13	−15
High water..	− 7½	− 4½	− 1½	+ 1½	+ 4½	+ 7½	+10¼	

In the Appendix, where the parallax corrections for the separate hours are given, it will be seen that although the *mean* parallax correction for parallax 57′½ is very small, and may almost be taken as 0, the parallax correction for the different hours for this value of the parallax, ranges from − 4·4 inches to + 3·7 inches. This arises from the circumstance that the range of parallax at different hours is not the same, owing to the moon's *variation.* By reason of the sun's action upon her, her orbit is an oval, the smaller axis of which is in the direction of the sun. Hence at syzygy she comes so near the earth, that her parallax amounts to 61′½; but at quadratures her parallax never exceeds 59′½. Consequently the mean parallax at syzygy is about 58′, and at quadrature about 57′. Hence if we take 57′½ for the mean parallax, the semimenstrual curve, obtained as above, is affected by a parallax correction, which is − at 0^h, and + at 6^h transit. If we take away this correction, so as to obtain the true mean semimenstrual inequality, we find the following.

Semimenstrual Inequality of Low Water at Plymouth, for the Mean Parallax and Declination.

Transit	h m 0 30	h m 1 30	h m 2 30	h m 3 30	h m 4 30	h m 5 30	h m 6 30	h m 7 30	h m 8 30	h m 9 30	h m 10 30	h m 11 30
H. low water	ft. in. 2 7	ft. in. 2 11½	ft. in. 3 9	ft. in. 4 10½	ft. in. 5 9½	ft. in. 6 6	ft. in. 6 1½	ft. in. 5 2	ft. in. 4 3	ft. in. 3 3	ft. in. 2 8	ft. in. 2 4

The total amount of this semimenstrual inequality is now 50 inches; and as the total inequality for high water is 37 inches, the total semimenstrual inequality for mean water amounts to 6½ inches.

4. *The Declination Correction of the Height of Low Water at Plymouth.*

The declination correction is obtained in a manner analogous to the parallax correction, from each year's observations. But the correction thus obtained is that which supposes the mean declination of each year to require no correction. Now this mean declination in different years is, as we have said, different. Therefore the declination correction so calculated will be different in different years; and hence we should require different declination tables in different positions of the moon's nodes. But the semimenstrual inequality is also different in different years, in virtue of the difference of the moon's mean declination. And when we take from the semimenstrual inequalities that which is requisite to reduce them to the mean declination 16½°, and add it to the declination corrections for each year, the declination corrections for different years coincide very nearly. For this purpose we add one inch to 1834, two inches to 1835, three inches to 1836, three inches to 1837, four inches to 1838, which were subtracted before. In this way we obtain the following results. These are the mean declination corrections, no account being taken of the difference of hours, which produces little effect.

I also add the declination corrections as obtained for high water at Plymouth. By comparison it will appear that the low water corrections are larger, especially for the high declinations.

Declination Correction. Plymouth.

Declination......	0° to 3°	3° to 6°	6° to 9°	9° to 12°	12° to 15°	15° to 18°	18° to 21°	21° to 24°	24° to 27°
	in.	in.	in.	in.	in.	in.	in.	in.	in.
Low water 1833..	−5½	−4½	−4½	−2	−1½	− ½	+2½	+5½	
1834..	−6	−7	−5	−4	−1	+ ½	+2½	+7	+11
1835..	−5	−4½	−5	−2	−1½	+ 1	+3	+6	+ 9½
1836..	−6½	−7	−4½	−4	−1	0	+4	+7	+11
1837..	−3½	−5	−3½	−2½	−2	+ ½	+4	+6	+ 9
1838..	−3½	−5	−2½	−2½	−1½	+ ½	+5	+6½	+12
Mean	−5	−5½	−4¼	−2¾	−1½	+ ½	+3½	+6½	+10½
High water......	+4¾	+3¾	+3	+2	+1	0	−1½	−3	− 5

There can be little doubt that the correction, as here given, for low water is more exact than that for high water, the process by which it is deduced having been applied in a more regular manner. And I may further observe, that the discussions, of which I have now been stating the results, remove all doubt on the question whether the declination correction, empirically deduced, varies as the square of the declination. The correction for low water given above, follows that law with great precision, as appears thus. The above corrections, reduced to 40ths of an inch and to declination 0, are as follows; and the squares of the corresponding declinations are expressed in the line below.

Correction . . .	10	− 10	40	100	150	230	350	470	630
Square of decl. .	1	16	49	100	169	256	361	484	625

I expect shortly to be able to give, as a Sequel to this memoir, a discussion of the *times* of low water at Plymouth corresponding to this discussion of the heights.

Postscript.

As a further proof how very nearly constant is the height of mean water, I annex the result of one year's observations made at Dundee, discussed by Mr. Dessiou. It will be seen that the differences are confined within 1½ inch, except at 11 and 12 o'clock, when they become about two inches more. This is in a (spring) tide of fourteen feet.

Dundee Tide Observations, 1837. Semimenstrual Inequality, Height of High and Low Water, and Mean Height.

Moon's Transit.	Interval between Moon's Transit and High Water.	Height of High Water.	Height of Low Water.	Height of Mean Water.
h m	h m	ft. in.	ft. in.	ft. in.
0	2 35	17 6·4	3 8·7	10 7·5
30	2 26·5	17 7·5	3 6	10 6·7
1 0	2 19·5	17 7·6	3 5	10 6·3
1 30	2 12·6	17 7·2	3 5·3	10 6·2
2 0	2 6	17 6·5	3 6	10 6·2
2 30	1 58·5	17 5·2	3 7·3	10 6·2
3 0	1 51	17 2	3 9	10 5·5
3 30	1 43·4	16 9·4	3 11·5	10 4·2
4 0	1 38	16 5	4 5·2	10 5·1
4 30	1 32·8	16 0·7	4 11·0	10 5·8
5 0	1 30·2	15 9·7	5 3·1	10 6·4
5 30	1 31·2	15 6·6	5 7·3	10 6·9
6 0	1 34·5	15 1·4	5 11·8	10 6·6
6 30	1 41·2	14 8	6 4·2	10 6·1
7 0	1 56	14 6·8	6 6	10 6·4
7 30	2 14·6	14 7	6 6·5	10 6·7
8 0	2 30	14 10·2	6 3·3	10 6·7
8 30	2 42	15 2	5 10·3	10 6·1
9 0	2 50·2	15 6	5 5·5	10 5·7
9 30	2 54	15 10	5 1·4	10 5·7
10 0	2 53·8	16 3·5	4 9	10 6·2
10 30	2 51	16 9·3	4 3·5	10 6·4
11 0	2 48	17 2	4 2	10 8
11 30	2 43·5	17 5	4 0·4	10 8·7

APPENDIX,

Showing the results of the Calculations on which the preceding Memoir is founded.

Plymouth. Heights of Low Water. Semimenstrual Line.

	h m 0 30	h m 1 30	h m 2 30	h m 3 30	h m 4 30	h m 5 30	h m 6 30	h m 7 30	h m 8 30	h m 9 30	h m 10 30	h m 11 30
	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.
1833.	2 1	2 7	3 6	4 8	5 9½	6 7	6 5	5 5½	4 3	3 2	2 4	2 0
1834.	2 3	3 0	3 9	4 9	5 10½	6 8	6 6	5 4	4 2	3 3	2 6	2 1
1835.	2 5	2 11	3 8	4 10	5 10½	6 9	6 6	5 6½	4 5½	3 4	2 8	2 3½
1836.	2 5½	3 1	3 11½	5 2	6 3	7 1	6 9	5 8	4 6½	3 5	2 8½	2 4
1837.	2 3	2 10	3 8	4 10	6 1	6 9	6 6	5 5	4 3	3 2	2 7	2 3
1838.	2 8½	3 3	4 2	5 3	6 3	7 0	6 11½	5 9	4 8	3 7	3 0½	2 9

	Declination.									Parallax.							
	0h 30m.									0h 30m.							
	0° to 3°	3° to 6°	6° to 9°	9° to 12°	12° to 15°	15° to 18°	18° to 21°	21° to 24°	24° to 28°	54′½	55′½	56′½	57′½	58′½	59′½	60′½	61′½
	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.
1833.	+ 3·2	+ 9·5	+ 8·6	+ 2·5	− 3·3	+ 3·9	− 8·3	− 3·4		− 6·9	− 7·8	− 5·0	− 2·9	− 1·8	+ 8·2	+12·7	+14·6
1834.	+11·4	+ 6·7	+ 3·3	+ 6·4	+ 7·8	− 3·0	− 2·0	− 8·9	−11·7	− 6·5	−16·0	−12·8	− 7·3	− 2·5	+ 6·5	+10·7	+21·7
1835.	+ 9·5	+ 2·8	+ 8·2	+ 7·2	+ 1·8	+ 3·0	− 4·0	− 4·9	− 7·1	−12·9	− 3·4	− 1·8	− 2·8	+ 6·2	+11·0	+14·8	+ 9·1
1836.	+12·0	+ 9·7	+ 4·0	− 0·3	+ 3·1	+ 9·0	+ 3·1	+ 1·2	−11·6	−15·9	− 0·5	+ 5·6	− 0·5	+10·5	+10·0	+ 8·7	− 1·0
1837.	+ 5·4	+ 9·5	+11·6	+ 4·8	+ 6·6	+ 4·4	− 3·9	−10·5	− 6·5	−12·2	− 6·9	− 7·3	− 0·5	+ 9·4	+ 8·5	+10·2	+12·3
1838.	+ 5·0	+ 8·7	+10·2	+ 5·7	+ 6·0	+ 6·0	+ 1·2	+ 1·0	− 9·3	− 2·6	−10·0	− 9·6	−12·3	− 7·5	+ 3·3	+10·8	+19·9
	+ 7·8	+ 7·8	+ 7·6	+ 4·4	+ 3·7	+ 3·9	− 2·3	− 4·3	− 9·2	− 9·5	− 7·4	− 5·1	− 4·4	+ 2·4	+ 7·9	+11·3	+12·8
	1h 30m.									1h 30m.							
1833.	+16·0	+ 1·5	+ 2·2	+ 4·2	+ 6·0	− 2·8	− 1·2	− 8·7		− 9·0	− 7·3	− 6·7	− 6·6	+ 1·7	+ 5·2	+14·6	+15·7
1834.	+ 6·0	+15·8	+ 7·5	+ 3·5	+ 2·0	+10·2	− 9·7	− 6·7	− 7·6	− 5·3	−13·1	− 5·3	− 8·3	+ 2·0	+ 2·3	+14·0	+25·3
1835.	+ 7·3	+11·5	+ 8·7	+ 0·3	+ 7·0	+ 2·0	− 7·0	− 8·8	− 6·0	−12·9	− 2·2	+ 1·5	− 1·6	+ 5·8	+13·0	+ 9·6	+12·7
1836.	+ 8·0	+11·7	+10·6	+ 9·0	+ 5·0	+ 6·3	− 2·1	− 4·3	− 9·8	−11·7	− 3·7	+ 5·3	+14·7	+ 3·6	+ 7·3	+ 4·1	+ 1·0
1837.	+ 6·2	+ 4·8	+ 7·0	+ 6·2	+ 5·7	+ 1·7	− 2·4	− 0·8	− 6·4	− 8·3	− 5·2	− 4·4	− 2·3	+ 1·5	+ 7·6	+13·3	+14·0
1838.	+ 7·0	+ 7·5	+ 1·0	+ 6·8	+11·4	+11·4	+ 0·9	+ 1·3	−10·7	− 3·6	−10·3	−15·7	− 9·0	− 7·6	+ 4·8	+17·3	+18·0
	+ 8·4	+ 8·8	+ 6·2	+ 5·0	+ 6·2	+ 4·8	− 3·6	− 4·7	− 8·1	− 8·5	− 7·0	− 4·2	− 2·2	+ 1·2	+ 6·7	+12·1	+14·5
	2h 30m.									2h 30m.							
1833.	+ 0·2	+13·7	+10·0	− 2·8	+ 2·5	+ 2·1	− 2·4	− 7·6		− 6·3	− 8·6	− 7·6	+ 2·0	+ 0·4	+ 8·8	+12·5	
1834.	+ 9·6	+ 4·0	+ 5·0	+13·0	+ 4·8	− 2·7	− 2·7	− 7·0	− 8·9	− 4·3	− 8·9	− 4·2	− 4·0	− 2·7	− 1·8	+18·6	
1835.	+10·6	+ 4·6	+11·6	+ 8·0	+ 7·4	− 1·2	− 0·4	− 5·9	−10·4	−12·7	− 0·5	0	+ 6·7	+ 1·5	+ 9·4	+10·5	
1836.	+12·5	+12·8	+ 8·0	+12·0	+ 6·2	+ 2·2	− 1·0	− 6·3	−10·7	−13·6	− 2·7	+11·0	+14·2	+ 6·8	+ 3·4	+ 1·8	
1837.	+ 8·0	+ 6·3	+ 1·6	+ 0·2	+ 3·2	+ 4·4	+ 3·2	− 1·6	− 5·9	− 9·1	− 5·6	− 1·2	− 2·2	− 1·8	+ 8·7	+15·3	
1838.	+ 0·7	+ 7·8	+18·3	+ 6·3	+ 9·6	+ 0·6	+ 8·0	+ 1·8	−10·4	− 1·7	− 7·4	−14·0	− 8·6	− 3·2	+ 6·8	+19·1	
	+ 6·9	+ 8·2	+ 9·1	+ 6·1	+ 5·6	+ 0·9	+ 0·8	− 4·4	− 9·3	− 8·0	− 5·6	− 2·7	+ 1·3	+ 0·2	+ 5·9	+13·0	
	3h 30m.									3h 30m.							
1833.	+ 5·4	− 0·2	+ 6·9	+ 6·2	+ 5·6	+ 2·2	− 7·4	− 4·2		− 3·7	− 6·4	− 5·2	− 1·9	+ 6·3	+ 8·4	+14·5	
1834.	+ 7·7	+ 9·8	+ 6·3	− 0·2	− 3·8	+ 3·5	+ 3·0	− 3·8	−13·4	− 5·5	− 7·0	− 8·6	− 3·7	− 5·8	+12·8	+22·2	
1835.	+ 6·3	+ 7·0	+12·4	+11·7	+ 2·2	− 0·8	+ 5·0	− 0·9	−11·4	− 8·6	− 2·5	+ 4·0	+ 4·0	+10·2	+ 3·7	+ 5·0	
1836.	+14·5	+12·8	+ 6·2	+ 7·4	− 1·2	+ 2·0	+ 0·1	− 3·4	− 8·2	−11·4	− 2·0	+ 3·7	+12·1	+ 8·8	+ 2·6	+ 5·7	
1837.	+ 0·3	+ 2·8	+ 6·8	+ 9·0	+ 5·0	+ 6·0	− 2·8	− 5·9	− 3·6	− 6·7	− 6·4	+ 0·3	− 5·7	− 1·0	+10·7	+21·3	
1838.	+17·3	+13·7	+ 8·0	+ 5·5	+ 6·2	+ 4·7	+ 7·0	− 4·3	−11·3	− 0·1	− 8·7	− 8·4	−13·4	− 3·0	+11·7	+26·8	
	+ 8·6	+ 7·6	+ 7·8	+ 6·6	+ 2·3	+ 2·9	+ 0·7	− 3·8	− 9·6	− 6·0	− 5·5	− 2·4	− 1·4	+ 2·6	+ 8·3	+15·9	

TABLE (Continued).

	Declination. $4^h\ 30^m$.									Parallax. $4^h\ 30^m$.							
	0° to 3°	3° to 6°	6° to 9°	9° to 12°	12° to 15°	15° to 18°	18° to 21°	21° to 24°	24° to 28°	54′½	55′½	56′½	57′½	58′½	59′½	60′½	61′½
	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.		
1833.	+6·2	+7·2	+5·2	+1·3	+1·8	+0·6	−0·7	−8·3		−4·9	−7·8	−6·3	+3·3	+3·3	+14·0		
1834.	+0·2	+6·8	+7·0	+7·0	+3·6	+5·5	−1·5	−6·0	−15·0	−6·4	−4·2	−6·3	−6·1	+3·8	+14·5		
1835.	+14·0	+9·5	+2·0	+4·1	+8·0	+6·8	+0·2	−7·9	−10·1	−7·7	−2·9	−0·3	+7·6	+10·5	+2·4		
1836.	+10·3	+8·4	+7·2	+7·8	+6·5	+0·8	−1·2	−7·3	−7·4	−13·5	−1·6	+8·5	+8·0	+9·1	+3·1		
1837.	+10·5	+8·8	+0·5	+2·6	−0·6	−3·0	−1·1	+5·7	−1·4	−7·0	−2·0	−6·0	−1·2	+5·5	+14·7		
1838.	+10·0	+17·7	+10·4	+13·5	+7·2	+0·6	−1·7	−7·8	−9·0	+4·2	−7·5	−10·0	−9·5	+3·8	+16·0		
	+8·5	+9·7	+5·4	+6·0	+4·4	+1·9	−1·0	−5·3	−8·6	−5·9	−4·3	−3·4	+0·4	+6·0	+10·8		
	$5^h\ 30^m$.									$5^h\ 30^m$.							
1833.	+6·8	+11·0	+4·2	−3·5	−1·7	+2·2	−2·4	−4·6		−4·9	−8·1	−4·4	−0·6	+8·2	+13·3		
1834.	+7·4	+4·8	+5·6	+7·5	+3·5	−0·9	−4·3	−5·5	−7·7	−6·3	−6·7	−5·6	−2·0	+8·9	+17·7		
1835.	+8·0	+11·4	+15·7	+5·6	+4·5	−0·6	−0·7	−5·6	−9·1	−7·6	−7·5	+5·7	+0·3	+2·1	+10·9		
1836.	+12·3	+18·5	+10·0	+8·8	+8·7	+6·0	−4·6	−6·9	−9·1	−14·3	+4·7	+10·7	+14·8	+13·3	+0·6		
1837.	+10·0	+9·0	+9·4	+0·3	+5·0	+7·6	−1·0	−8·7	−6·1	−6·3	−4·8	−3·0	+2·9	+3·3	+14·7		
1838.	+11·8	+14·8	+8·0	+4·6	+7·8	+5·8	+2·4	−6·8	−10·7	+1·1	−8·8	−11·9	−7·4	+9·7	+16·0		
	+9·4	+11·6	+8·8	+3·9	+4·6	+3·4	−8·4	−6·4	−8·5	−6·4	−5·2	−1·4	+1·3	+7·6	+12·2		
	$6^h\ 30^m$.									$6^h\ 30^m$.							
1833.	+4·0	+1·4	+9·0	+9·0	−1·8	−2·9	−2·7	−5·5		−7·4	−7·3	+0·1	+6·0	+4·1	+13·9		
1834.	+8·4	+13·4	+6·2	−2·2	−0·2	+1·0	−4·1	−2·4	−9·5	−2·3	−8·4	−7·7	−1·9	+8·2	+16·3		
1835.	+7·8	+6·2	+3·0	+3·7	+3·7	+2·0	−4·0	−3·0	−9·0	−8·1	−3·6	−4·1	+5·7	+3·6	+13·7		
1836.	+10·5	+7·8	+5·5	+4·7	−0·2	+2·8	+4·0	−0·9	−9·1	−14·7	+1·4	+7·8	+10·0	+8·3	+5·8		
1837.	+5·0	+10·3	+14·2	+10·7	+6·0	−1·6	−3·0	+0·4	−8·9	−8·2	−6·7	+2·7	+6·2	+6·8	+8·1		
1838.	+10·6	+2·0	+8·0	+5·2	+3·4	+2·5	−1·1	+0·1	−6·5	−1·1	−7·1	−6·8	−3·7	+9·1	+10·9		
	+7·7	+6·9	+7·6	+5·2	+1·8	+0·6	−1·8	−1·9	−8·6	−7·0	−5·3	−1·3	+3·7	+6·7	+11·5		
	$7^h\ 30^m$.									$7^h\ 30^m$.							
1833.	+1·0	+7·2	+4·5	+1·7	+4·9	+4·2	−4·3	−6·0		−10·5	−5·6	+1·7	+4·8	+5·3	+11·6		
1834.	+6·2	+5·3	+1·0	+7·0	−3·3	−2·1	+1·3	−3·9	−7·0	−5·1	−8·9	−11·0	−5·8	+5·1	+14·9		
1835.	+12·3	+1·5	+1·2	−2·8	+1·8	+3·3	+1·3	−1·0	−4·5	−8·7	−7·5	−2·4	+2·6	+9·0	+14·7		
1836.	+2·5	+3·3	+8·5	+9·0	+3·6	+4·2	−6·4	+1·0	−6·1	−13·1	−3·7	+0·6	+13·1	+8·6	+4·3		
1837.	+5·7	+9·7	+0·8	+1·0	+7·4	+8·5	−3·3	−5·4	−3·8	−8·1	−7·8	−0·3	−0·3	+8·3	+9·0		
1838.	−8·3	+1·2	+4·5	+4·0	+5·2	+4·5	+4·2	+0·5	−5·0	−1·0	−7·0	−7·6	+2·7	+1·3	+7·1		
	+3·2	+4·7	+3·1	+3·3	+2·3	+3·8	−1·2	−2·5	−5·3	−7·8	−6·8	−3·2	+2·9	+6·3	+10·3		
	$8^h\ 30^m$.									$8^h\ 30^m$.							
1833.	+8·4	−0·6	−2·0	+0·3	−0·3	+2·1	−0·9	−3·4		−9·9	−6·1	−1·3	−1·7	+11·7	+8·6	+17·0	
1834.	+11·0	+1·9	+3·8	+5·0	+6·6	−4·3	−3·0	−3·4	−9·3	−3·7	−9·3	−12·3	−9·0	+1·2	+13·9	+19·5	
1835.	+5·2	+3·3	+12·5	+5·2	−2·2	+1·0	−0·7	+0·9	−5·7	−11·0	−5·3	−2·5	−0·1	+9·6	+13·0	+15·0	
1836.	+9·0	+10·8	+5·2	+3·0	+5·5	+2·7	−1·9	−5·4	−4·5	−12·3	−5·8	+7·3	+9·4	+9·0	+6·6	+9·8	
1837.	+4·8	+11·7	+9·7	+4·0	+4·0	−3·5	+5·6	+0·7	−6·0	−10·7	0	−1·8	+7·0	+6·8	+9·2	+10·6	
1838.	+3·0	−1·7	0	+3·0	+1·8	+2·0	+4·8	+6·5	−4·3	−2·6	−7·4	−1·4	−6·6	+2·8	+5·8	+13·3	
	+6·9	+4·3	+4·9	+3·4	+2·6	0	+0·7	−0·7	−6·0	−8·4	−5·6	−2·0	−0·2	+6·9	−9·5	+14·2	
	$9^h\ 30^m$.									$9^h\ 30^m$.							
1833.	+9·0	+3·8	−3·0	+5·0	+0·3	−1·8	−0·5	−3·9		−9·6	−4·8	−6·7	+1·7	+4·3	+9·5	+12·9	
1834.	+5·8	+15·2	+15·5	+1·5	−0·6	−1·0	−3·9	−6·1	−9·5	−1·3	−11·7	−11·2	−11·1	−4·2	+4·6	+20·8	
1835.	+4·8	+3·8	−0·5	−0·4	+7·6	−0·7	+0·9	−3·3	−11·2	−12·2	−8·0	−2·6	−1·7	+6·4	+10·2	+14·2	
1836.	+8·5	+10·6	+9·7	+7·3	+2·4	−0·9	−0·4	−3·7	−6·8	−12·9	−6·4	−0·3	+11·6	+10·0	+9·7	+9·2	
1837.	+11·3	+4·6	+2·7	+7·0	+13·2	+6·7	−5·7	−1·6	−7·1	−12·7	−2·6	+5·7	−2·6	+2·0	+11·7	+10·5	
1838.	+10·2	+10·4	+3·0	+5·4	+0·9	+1·7	−2·5	−1·3	−6·5	−6·3	−8·0	−5·5	−8·0	−7·5	+6·5	+14·5	
	+8·3	+8·1	+4·6	+4·3	+4·0	+0·7	−2·0	−3·3	−8·2	−9·2	−6·9	−3·4	−1·7	+1·8	+8·7	+13·7	

TABLE (Continued).

	Declination. $10^h 30^m$.									Parallax. $10^h 30^m$.							
	0° to 3°	3° to 6°	6° to 9°	9° to 12°	12° to 15°	15° to 18°	18° to 21°	21° to 24°	24° to 28°	54′½	55′½	56′½	57′½	58′½	59′½	60′½	61′½
	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.
1833.	− 1·7	+ 7·0	+ 5·8	− 1·7	− 5·8	+ 5·5	− 3·5	− 1·6		− 9·2	− 7·6	− 5·7	− 4·2	+ 2·7	+ 8·0	+13·5	+20·7
1834.	+13·0	+ 4·3	+ 0·8	+ 8·2	+ 8·7	− 2·9	− 0·7	− 6·1	−12·7	− 4·5	− 5·0	−12·7	−14·5	− 5·0	− 0·1	+16·4	+27·0
1835.	+ 2·6	+ 8·2	+ 7·8	+ 7·0	+ 1·7	− 3·8	+ 0·4	− 5·0	− 4·3	−12·9	− 7·3	− 8·0	+ 3·4	+ 5·3	+11·2	+14·1	+15·0
1836.	+10·3	+ 8·6	+ 5·5	+ 4·5	− 0·7	+ 3·2	− 1·5	− 7·7	− 4·8	−13·1	− 5·8	+ 2·7	+ 3·3	+10·6	+12·8	+ 7·0	+ 9·0
1837.	− 1·0	+ 9·5	+13·8	+ 8·4	− 1·2	+ 5·1	+ 2·0	−12·3	− 6·3	−13·3	− 3·8	− 0·4	+ 3·4	+ 3·3	+ 8·2	+14·1	+13·3
1838.	+ 5·5	+10·0	+10·2	+14·0	+ 1·8	+ 1·7	− 3·6	− 5·6	− 6·0	− 7·2	− 5·4	− 6·0	− 8·0	− 2·6	+ 0·6	+11·9	+28·7
	− 4·8	+ 7·9	+ 7·3	+ 6·7	+ 0·8	+ 1·5	− 1·1	− 6·4	− 6·8	−10·0	− 5·8	− 5·0	− 2·8	+ 2·7	+ 6·8	+12·8	+19·0
	$11^h 30^m$.									$11^h 30^m$.							
1833.	+ 9·0	− 0·6	+ 1·7	+ 5·5	+ 9·4	− 4·3	+ 0·2	− 5·8		− 8·2	− 6·8	−12·0	− 1·2	− 0·2	+ 2·7	+10·7	+16·9
1834.	+ 3·3	+10·4	+15·7	+ 2·8	− 0·9	+ 8·1	− 4·0	− 9·4	−13·3	− 6·0	−12·4	−19·3	−13·5	− 5·0	− 0·2	+11·4	+21·1
1835.	+ 6·3	+ 8·7	+ 7·7	− 1·2	+ 1·7	+ 3·0	+ 0·6	− 3·9	− 6·1	−12·3	− 1·7	− 8·7	+ 2·1	+ 8·2	+ 8·1	+18·3	+ 7·3
1836.	+ 7·0	+ 8·7	+ 8·0	+ 9·5	+ 8·2	− 2·0	− 2·6	− 4·8	− 7·6	−13·2	− 5·1	− 0·5	+ 5·4	+16·2	+10·9	+ 6·4	+ 5·3
1837.	+16·8	+16·0	− 1·4	+ 9·2	+ 6·3	− 6·2	+ 1·2	+ 5·2	− 9·9	−14·7	− 3·2	− 5·0	+ 1·6	+ 5·0	+10·3	+13·2	+12·5
1838.	+21·3	+14·0	− 0·7	+ 6·2	+ 5·2	+ 2·2	− 5·5	− 8·6	− 4·8	− 6·3	− 5·6	− 8·5	− 9·5	− 0·8	− 9·0	+11·7	+22·1
	+10·6	+ 9·5	+ 5·2	+ 5·3	+ 5·0	+ 0·1	− 1·7	− 4·6	− 8·3	−10·1	− 5·8	− 9·0	− 2·5	+ 3·9	+ 3·8	+12·0	+14·2

Declination. Mean for each Hour of Transit.

1833.											
h m 0 30	h m 1 30	h m 2 30	h m 3 30	h m 4 30	h m 5 30	h m 6 30	h m 7 30	h m 8 30	h m 9 30	h m 10 30	h m 11 30
ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.
14 5	14 5	14 37	14 4	14 44	14 42	13 49	13 1	14 15	14 8	14 17	15 1
14 10	13 20	14 12	14 19	14 36	15 15	12 25	14 26	13 16	14 34	14 21	14 23
14 36	14 18	14 9	15 18	13 26	15 29	13 56	14 51	13 44	14 12	12 59	14 54
14 15	14 17	14 54	14 18	14 57	12 37	15 44	14 0	14 53	13 12	14 23	13 52
14 17	14 0	14 28	14 30	14 26	14 31	13 59	14 5	14 2	14 2	14 0	14 32
1834.											
15 58	14 10	16 5	14 22	16 13	14 47	16 10	16 14	14 23	15 56	14 37	16 1
14 46	16 7	14 44	15 57	14 39	16 2	16 6	14 58	15 32	14 22	15 55	13 58
13 54	16 36	14 26	16 42	14 15	15 27	14 20	15 2	14 32	16 29	16 13	14 35
15 37	15 11	16 20	15 1	15 15	15 16	14 12	14 6	16 5	16 3	15 24	14 8
15 4	15 31	15 24	15 31	15 6	15 23	15 12	15 5	15 8	15 43	15 32	14 41
1835.											
17 1	16 22	15 56	15 42	16 29	15 45	15 22	16 42	15 38	16 29	14 59	16 56
15 55	17 59	15 26	17 12	15 33	16 17	15 30	15 22	16 49	15 13	16 34	14 59
15 36	16 22	15 40	17 19	15 43	18 13	16 36	15 39	17 32	14 40	16 45	16 13
17 26	15 26	16 36	15 43	17 26	16 42	16 13	16 46	16 3	17 2	14 30	17 37
16 30	16 32	15 55	16 29	16 18	16 44	15 55	16 7	16 31	15 51	15 42	16 26

TABLE (Continued).

1836.											
h m 0 30	h m 1 30	h m 2 30	h m 3 30	h m 4 30	h m 5 30	h m 6 30	h m 7 30	h m 8 30	h m 9 30	h m 10 30	h m 11 30
ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.
17 27	15 33	18 1	16 15	18 26	16 54	18 41	16 32	17 9	18 4	15 55	17 50
15 40	18 45	15 23	18 59	15 46	18 31	16 14	18 6	15 51	18 4	17 9	17 45
17 55	17 39	17 5	17 53	16 11	18 18	16 41	17 10	17 15	16 14	18 33	15 24
17 56	16 20	18 35	16 24	17 27	17 39	16 59	16 40	18 4	16 40	16 27	17 16
17 15	17 4	17 16	17 23	16 58	17 50	17 9	17 7	17 5	17 16	17 1	17 4
1837.											
18 11	17 28	16 49	18 13	18 17	18 16	19 7	17 18	19 38	16 12	17 17	16 27
18 21	16 52	17 25	16 37	18 39	18 44	18 33	18 32	17 11	18 1	16 36	18 36
16 59	17 2	18 3	16 47	19 19	18 14	15 30	18 26	17 2	17 18	18 1	16 56
15 22	17 19	16 32	18 39	18 28	16 45	17 55	17 16	18 11	17 21	17 54	16 45
17 9	17 10	17 12	17 34	18 41	18 0	17 46	17 58	18 1	17 13	17 27	17 11
1838.											
17 55	18 31	17 14	18 7	17 40	16 3	18 15	17 8	19 5	16 56	19 20	18 22
16 52	18 0	17 20	18 13	17 23	17 18	16 58	18 23	17 34	18 32	18 2	18 22
17 50	17 11	16 28	18 23	17 27	19 6	17 9	18 37	17 14	17 48	17 32	17 51
17 15	18 14	17 36	18 22	18 38	18 0	18 52	17 9	18 19	17 1	18 20	18 8
17 28	17 59	17 10	18 16	17 47	17 37	17 49	17 49	18 3	17 34	18 19	18 11

XII. *Researches on the Tides.—Eleventh Series. On certain Tide Observations made in the Indian Seas. By the Rev.* W. Whewell, *B.D. F.R.S., Fellow of Trinity College, Cambridge.*

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CERTAIN series of tide observations, made at several places in the Indian Seas, having been forwarded to the Admiralty by the Honourable East India Company, I examined these by the assistance of Mr. D. Ross of the Hydrographer's Office. The observations were very incomplete, as the following account of them will show. But as the tides of those seas offer some very curious phenomena, I endeavoured to discover how far these phenomena were illustrated by the observations thus sent; and I now lay the results of this examination before the Society, in order that they may be preserved, and combined with any information obtained hereafter from these seas. The places of observation were

	Lat. N. °	'	Long. E. °	'
Coringa Bay, on the coast of Golconda . .	16	49	82	6
Cochin, on the Malabar coast	9	57½	76	29
Surat Roads, in the Gulf of Cambay . . .	21	11	73	5½
Gogah, on the opposite side of the Gulf of Cambay	21	41	72	23
Bassadore, at the western extremity of the Island of Kismis, at the entrance of the Persian Gulf	26	39	55	32

1. *At Cochin.*—Although there are two years' tides (1836, 1837) for this place, still they are only taken once in twenty-four hours; and on examination of the heights they seldom vary more than one foot from spring to neap, but the range is only three feet.

2. *Coringa River.*—The observations are also for two years, 1836 and 1837, at this place, but only once in twenty-four hours. These tides appear to be more carefully taken both in times and heights than at Cochin.

3. *In Surat Roads.*—The tides were taken on board an Indian brig, and then only when in harbour, commencing October 18, 1834, and continued to the 23rd. A gap takes place as the brig goes on a cruize till the 31st. Tides continued to Nov. 18; gap to 21st. Tides to December 20; gap to January 7, 1835. Tides to March 29; gap to April 5. Observations end 25th. These tides are for A.M. and P.M.

4. *At Gogah.*—These observations seem to be the most regular; they are taken

at A.M. and P.M.; but there are not four months of them, as they commence on the 10th of June, and end September 30, 1835.

5. *At Bassadore.*—The tides at this place are taken for a few days in September; a few days in October, and the whole of November for 1834.

In the first place, I proceeded to find the establishment of each place, and the semimenstrual inequality so far as the observations allowed. The following are the results.

	Lunitidal Interval.		Mean, or correct establishment.	Difference.
	Least.	Greatest.		
	h m	h m	h m	m
Coringa Bay ..	9 25	10 39 =	10 2	74
Cochin	0 32	2 21 =	1 26½	109
Surat	3 49	4 59 =	4 24	70
Gogah	4 6	5 27 =	4 46½	81

In the second place, it appeared that at the two places in the Gulf of Cambay, there is an enormous diurnal inequality of the heights, amounting at both Surat and Gogah to not less than seven or eight feet. The observations being laid down in curves, this feature was more marked than in any case which I have yet examined, even than Singapore, of which the curve was given in the Philosophical Transactions for 1837.

The observations give this inequality at Surat and Gogah somewhat irregularly, but not so much so as to prevent my obtaining its epoch in an approximate manner. It appears that the diurnal inequality disappears, and changes its sign, about two days after the moon's declination vanishes.

In the third place, it appeared that at Bassadore there is a very large diurnal inequality of the *times*, amounting to above two hours in some instances. This is a new case: for though I had already ascertained that in some places there is a diurnal inequality of the lunitidal intervals, I had never supposed that it could amount to a quantity so large as this, which indeed utterly displaces the tides. For instance, about the 23rd of November 1834, the tides on the afternoon of each day were earlier by about two hours than the hour of the tides in the forenoon. What makes this anomaly still more remarkable is, that at this place there is little or no diurnal inequality of the heights. As this result is a very novel one, I shall here give a copy of the original observations.

Bassadore Original Tide Register for the Month of November 1834.

	Low Water.		High Water.		Range.		Low Water.		High Water.		Range.
	Time.	Height.	Time.	Height.			Time.	Height.	Time.	Height.	
	h m	ft. in.	h m	ft. in.	ft. in.		h m	ft. in.	h m	ft. in.	ft. in.
Nov. 1 A.M.	5 35	1 6	11 58	9 0	7 6	Nov. 16 A.M.	5 55	3 0	11 55	9 0	6 0
P.M.	6 32	0 0	....	....	....	P.M.	6 43	0 6	13 12	8 6	8 0
2 A.M.	6 15	1 0	0 26	9 6	8 6	17 A.M.	6 17	2 0	11 34	8 6	6 6
P.M.	7 4	0 0	0 30	9 6	9 6	P.M.	6 35	1 6	13 22	9 0	7 6
3 A.M.	7 1	1 0	1 23	9 6	8 6	18 A.M.	4 46	2 0	....	....	....
P.M.	8 3	1 0	1 2	9 0	8 0	P.M.	7 44	3 0	37	9 0	6 0
4 A.M.	7 39	1 0	2 14	9 0	8 0	19 A.M.	7 24	3 0	2 12	8 6	5 6
P.M.	8 53	2 0	1 32	9 6	7 6	P.M.	8 20	1 6	1 6	8 6	7 0
5 A.M.	8 28	2 0	3 26	9 0	7 0	20 A.M.	7 57	2 0	2 55	9 0	7 0
P.M.	8 33	2 0	2 17	9 0	7 0	P.M.	9 7	2 0	1 35	9 0	7 0
6 A.M.	9 13	2 6	4 4	9 0	6 6	21 A.M.	8 48	3 0	3 57	8 6	5 6
P.M.	10 16	2 6	2 58	9 0	6 6	P.M.	9 44	2 6	2 8	9 0	6 6
7 A.M.	10 10	2 6	5 0	8 6	6 0	22 A.M.	9 16	2 0	4 34	8 6	6 6
P.M.	11 14	4 0	3 38	8 0	4 0	P.M.	10 30	3 0	2 38	8 6	5 6
8 A.M.	11 35	4 0	6 15	7 6	3 6	23 A.M.	13 13	4 0	5 33	8 6	4 6
P.M.	....	....	5 3	7 6		P.M.	11 39	4 0	3 41	8 0	4 0
9 A.M.	26	2 6	7 35	7 0	4 6	24 A.M.	11 27	4 0	6 43	8 0	4 0
P.M.	1 18	4 0	7 7	7 6	3 6	P.M.	....	....	5 7	8 6	
10 A.M.	2 48	3 0	8 41	7 0	4 0	25 A.M.	0 52	4 0	7 50	8 0	4 0
P.M.	2 9	3 0	7 27	7 0	4 0	P.M.	1 4	4 0	6 29	9 0	5 0
11 A.M.	2 23	3 0	9 40	8 0	5 0	26 A.M.	1 52	4 0	8 48	8 6	4 6
P.M.	4 3	3 0	9 41	8 0	5 0	P.M.	2 32	3 0	8 8	9 6	6 6
12 A.M.	3 50	3 0	10 30	8 0	5 0	27 A.M.	2 30	3 0	9 12	9 0	6 0
P.M.	4 58	3 0	10 25	8 0	5 0	P.M.	2 31	2 0	9 38	9 0	7 0
13 A.M.	5 4	3 0	10 42	9 0	6 0	28 A.M.	3 39	2 0	9 37	9 0	7 0
P.M.	6 25	3 0	11 7	9 0	6 0	P.M.	4 20	1 0	10 42	9 0	7 0
14 A.M.	4 23	4 0	10 44	9 0	5 0	29 A.M.	4 12	1 0	10 21	9 6	8 6
P.M.	5 43	3 0	11 54	9 0	6 0	P.M.	5 18	1 0	11 49	10 0	9 0
15 A.M.	5 15	3 0	11 26	9 0	6 0	30 A.M.	5 20	2 0	11 15	9 9	7 9
P.M.	6 8	3 0	12 29	9 0	6 0	P.M.	6 9	0 0	12 22	9 9	9 9

To this table there is no signature, but all the others for Bassadore are signed **Thomas Elroon**, Commodore.

In order to bring into view the diurnal inequality of the times in these observations, we take out the times of moon's transit, corrected for the longitude of Bassadore. We hence find the interval between each transit and the succeeding time of high water, as will be seen below; and it appears that the *lunitidal intervals* vary from 9^h 48^m to 13^h 33^m. At the beginning of the month the successive lunitidal intervals are equal. About the 7th they become alternately about 12^h and 10^h, or 13^h and 11^h; on the 12th they are again equal; about the 19th they again become alternately about 12^h and 10^h. The difference gradually diminishes to the 20th, when they are again equal; after which the inequality reappears, and continues to the end of the month.

This diurnal inequality of above two hours in the time of high water, in a situation in which the diurnal inequality of height is insensible, I cannot but consider as a most curious tidal feature in addition to those already remarked in the Indian Seas.

	Moon's Transit.	Succeeding Tide.	Lunitidal Interval.		Moon's Transit.	Succeeding Tide.	Lunitidal Interval.
	h m	h m	h m		h m	h m	h m
1834. 31	23 48	11 58	12 10	Nov. 16	12 5	13 12	13 7
Nov. 1					12 28	11 34	11 6
	16	26	12 10	17	12 52	13 22	12 30
2	45	30	11 45		13 17	37	11 20
	1 14	1 23	12 9	18	13 42	2 12	12 30
3	1 44	1 2	11 18		14 8	1 6	10 58
	2 14	2 14	12 0	19	14 35	2 55	12 20
4	2 44	1 32	10 48		15 2	1 35	10 33
	3 14	3 26	12 12	20	15 29	3 57	12 28
5	3 43	2 17	10 34		15 56	2 8	10 12
	4 12	4 4	11 52	21	16 23	4 34	12 11
6	4 41	2 58	10 17		16 50	2 38	9 48
	5 8	5 0	11 52	22	17 17	5 33	12 16
7	5 35	3 38	10 3		17 43	3 41	9 58
	6 1	6 15	12 14	23	18 8	6 43	12 35
8	6 26	5 3	10 37		18 34	5 7	10 33
	6 49	7 35	12 46	24	18 59	7 50	12 51
9	7 12	7 7	11 55		19 24	6 29	11 5
	7 34	8 41	13 7	25	19 49	8 48	12 59
10	7 56	7 27	11 31		20 14	8 8	11 54
	8 17	9 40	13 23	26	20 40	9 12	12 32
11	8 37	9 41	13 4		21 6	9 38	12 32
	8 57	10 30	13 33	27	21 32	9 37	12 5
12	9 17	10 25	13 8		21 59	10 42	12 43
	9 37	10 42	13 5	28	22 26	10 21	11 55
13	9 57	11 7	13 10		22 54	11 49	12 55
	10 17	10 44	12 27	29	23 23	11 15	11 52
14	10 38	11 54	13 16		23 53	12 22	12 29
	10 59	11 26	12 27	30			
15	11 20	12 29	13 9				
	11 42	11 55	12 13				

XIII. *Account of Experiments on Iron-built Ships, instituted for the purpose of discovering a correction for the deviation of the Compass produced by the Iron of the Ships.* *By* George Biddell Airy, *Esq. A.M., Astronomer Royal.*

Received April 11,—Read April 25, 1839.

Section I.—*Preliminary.*

In the months of October and November 1835, a series of observations was made under the direction of the Board of Admiralty, by Commander Johnson, R.N., on the iron steam-ship Garry Owen, for ascertaining the amount of disturbance of the compass produced by the magnetic attraction of the iron, of which the ship's sides and bottom are composed. The details of these experiments are published in the Philosophical Transactions for 1836. Results of great importance were obtained as to the amount of deviation of the compass in different parts of the ship; and several remarkable experiments were described, which seemed to prove that the ship acted upon external compasses, in the manner of a permanent magnet. But no attempt was made to discover the laws of the magnetic disturbance, or to ascertain its causes; and no attempt could therefore be made to neutralize the ship's disturbing force by the introduction of new disturbing forces.

The last-mentioned point was, however, kept in sight by the Board of Admiralty, and partial arrangements were made for conducting a series of experiments referring expressly to this subject, whenever a favourable opportunity should occur. In the month of July 1838 the iron-built steam-ship the Rainbow was placed by the General Steam Navigation Company at the service of the Admiralty for magnetic examination. The conduct of the experiments was entrusted by the Board to me, and the vessel was immediately placed in the Basin of the Deptford Dock Yard.

The first point to be settled was the selection of stations in which the deviations of the compass should be observed. The object which I proposed to myself (the ascertaining the laws of the deviation, and the neutralization, if possible, of the deviating forces) made it a matter of no interest to me to try the action of the compass in many different places. I determined therefore on selecting only those in which it was likely that the compass, in the ordinary course of navigation, might be used for steering. With the assistance of Capt. Shirreff, R.N., Captain Superintendant of the Deptford Dock Yard, I fixed on the four following stations:

Station I., very near the binnacle as fixed in the ship, at the distance of 13 feet 2 inches from the extreme part of the stern.

Station II., at the place where the binnacle would probably be fixed in a steam-ship of the Royal Navy, at the distance of 31 feet 9 inches from the stern.

Station III., as near to the mizen-mast as observations could conveniently be made, distant 48 feet 3 inches from the stern.

Station IV., a short distance abaft the fore-mast, at the distance of 47 feet from the knight-head, or 151 feet 6 inches from the stern.

For the purpose of commanding a clear view above the engine-boxes and paddle-boxes, it was necessary to place in each of these positions a stage of considerable height (the elevation of its upper floor was 10 feet 2 inches). A lower floor was fixed in the stage at the elevation of 3 feet 6 inches above the deck; and upon this the azimuth compass used in the experiment was always placed. The elevation of its card above the deck was then about 4 feet ½ inch. This elevation was adopted as coinciding almost exactly with that at which the compass was mounted in the binnacle when the ship was placed under my management.

The trouble of observing a compass upon the top of the stage was so small, that (although unimportant for the special objects of this inquiry) it was thought desirable to ascertain its error, as well as that of the lower compass, in each position of the vessel's head. In the observations of the first day the upper compass was placed upon the stage, with its card raised 10 feet 8 inches above the deck; it was afterwards raised 2 feet higher.

In adopting a method of observation, the following considerations were taken into account.

If we were certain that no sensible effect would be produced on the compass by the iron in the wharfs and the various buildings surrounding the basin, the easiest method of determining the error of the ship's compass would be to compare it with a compass on shore, either by reciprocal observations, or by observing with both the line of the ship's fore-mast and mizen-mast, or by observing with the ship's compass a mark at some distance, and observing with the shore compass the azimuth of the same mark when seen in the same line with the ship's compass. I omit the mention of a mark at so great a distance that its parallax in the movement of the ship would be insensible, because no very distant mark can be seen from the Deptford Dock Yard. But these methods require a general certainty that there are no local disturbing causes. The vessel had been placed so early in the basin, that I had had no opportunity of examining into the existence of local disturbances; and, as far as the general aspect of the localities could suggest an opinion, it was extremely probable that the local disturbances would be sensible. There are several iron posts, and an iron crane, very near the basin wall; there was a great mass of iron tanks in a shed adjoining it; and the great length of the ship brought the compass when at Station I. very near to one side or another of the basin. It appeared therefore best to observe the azimuth of a chimney on the opposite side of the river, with the ship's compass only, and to register the local position of the compass in such a manner that the correction depending upon its place in the basin might be taken from a map; and it was supposed that by afterwards carrying a compass round the basin when the ship was removed, observing with it the same chimney, and correcting for local position in the same manner, the amount of error depending on local disturbance might be found, and applied, if necessary, to the observations in the ship.

The observations were made in the following manner.

Through a hole in the upper floor of the stage a wooden spindle, 2½ inches square, was passed. The lower end of this spindle was connected with a wooden fork, whose branches lodged upon the gimble-frame of the azimuth-compass, in such a manner that when the spindle was turned it carried with it the frame of the compass. At the upper end of the spindle was a small telescope with a thick wire in its field of view; the height of the telescope above the deck was about 15 feet 6 inches; so that a person standing on the upper floor of the stage could with the telescope conveniently observe the chimney above the paddle-boxes and engine-boxes, while a person below could read off the bearing by the azimuth compass in the usual way. At the beginning and at the end of each series, or at other convenient times, the bearing of the chimney was observed as well with the sights of the azimuth compass as with the telescope; and thus was obtained an index error of the telescope, by which all observations made with the telescope could be converted into equivalent observations with the compass sights.

Two theodolites were stationed at points commanding the chimney and every part of the basin. The duty of the theodolite observers was, at each position of the ship, to observe the wooden spindle placed on the compass, to observe the chimney, and to make reciprocal observations.

The person upon the top of the stage, having ascertained that the ship was steadily moored, directed that the ship's bell should be struck, as a signal to all the observers, and then gave out the ordinal number of the observation. This was entered by each of the observers in his book. The person on the stage directed his telescope to the chimney and gave notice thereof to the person below, who read off the compass and entered the reading in his book: then the person on the stage directed his telescope to the centre of the ship's funnel, and the compass in like manner was read by the person below: these observations were repeated; then the person at the top of the stage observed the chimney with the upper compass, and entered the reading in his book. During these observations, each of the theodolite-observers had twice observed the compass-spindle, the chimney, and the other theodolite, and entered the readings in his book; and other observations were made and entered by other persons, as will shortly be mentioned. Each observer gave signal of the completion of his observations by raising a flag: as soon as all had finished, the word was given for swinging the ship into a new position.

On the first two days of observation, July 26 and 30, a compass was placed upon one of the paddle-boxes, and the azimuth of the chimney was regularly observed with it. The disturbances of the compass, however, appeared anomalous; and as it did not appear probable that this compass could be corrected so completely as the others, the observations were never wholly reduced.

In the first two days there were mounted on shore, near the western side of the basin, a dipping needle, and a two foot horizontal needle, suspended by a skein of silk, and carrying a reflector with which the divisions of a scale were observed in GAUSS's method; and near the south side of the basin a dipping needle, and a two foot horizontal needle transverse to the meridian, carried by two parallel skeins of silk in the manner of GAUSS and WEBER's bifilar magnetometer, and carrying a reflector with which a divided scale was observed. These were observed when the signal was given for the compass observations.

On the third day, July 31, three dipping needles were placed in the ship, in positions very near to the compass stations I. III. IV.: during one complete revolution of the ship they were placed so that the vibrations of the needle were in the plane of the keel; and during another complete revolution, they were so placed that the vibrations were transverse to the keel.

The same dipping needles were afterwards, on August 2, carried on shore, and arranged in a line nearly E. and W. for observation of the effect produced by bringing the ship's head and stern very near to the side of the basin while the keel was E. and W. They were then arranged in a N. and S. line, and the effects of the two extremities of the ship while the keel was N. and S. were examined.

The ship was then taken out of the basin, and on August 6 the same compass which had been used at the lower stage-floor in the four stations on board the ship, was mounted upon a stage carried by a raft (in the construction of which there was not much iron) and floated round the basin. The height of the compass above water was made nearly the same as the height of the lower stage floor (in the ship) above water; and the raft was carried to all parts where there was suspicion of local disturbance, care being taken that it should be placed nearly opposite each iron post, and nearly intermediate to adjacent posts.

On August 14 observations of horizontal intensity were made with a needle suspended by a silk fibre, at each of the four stations, the ship's head being placed successively N., W., S., and E.*, by means of a compass on shore.

On August 20 and August 22, after the application of correctors, observations of the compasses were made in the same manner as those made for ascertaining the amount of error, in order to verify the accuracy of the correction.

I have now only to give the names of the persons employed in the various parts of the operation.

The swinging the ship round was managed by Captain SHIRREFF or by Mr. MORRICE

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* It is always to be understood that the cardinal points of the compass and the azimuths are referred to the *magnetic* meridian.

(Master Attendant of the Dock Yard), with the assistance of men attached to the Dock Yard.

The two theodolites (A. and B.) were assigned to Mr. JAMES GLAISHER and Mr. ELLIS, Assistants at the Royal Observatory.

The western dipping needle and horizontal needle (C.) were observed, on July 26, by Professor CHRISTIE, and on July 30 by the Rev. R. SHEEPSHANKS.

The southern dipping needle and horizontal needle transverse to the meridian (D.) were observed by the Rev. R. MAIN, First Assistant at the Royal Observatory.

The observations with the compass on the paddle-box (E.) were made by Captain SHIRREFF.

The upper telescope was directed and the upper compass (F.) read off by myself in the whole of the experiments.

The compass on the lower stage-floor (G.) was read off on July 26 by Lieutenant DENISON, R.E., on July 30 by FRANCIS BAILY, Esq., and in all the subsequent observations by Captain SHIRREFF.

The compass on the raft (H.) was observed by Mr. MAIN, with the assistance of Captain SHIRREFF.

Of the three dipping-needles on board, on July 31, the sternmost was read by Captain SHIRREFF, and the other two by Mr. MAIN.

The dipping needles on shore, on August 2, were read by Mr. MAIN.

The observations of the time of vibration for horizontal intensity on August 14 were made by myself, with the assistance of Captain SHIRREFF.

To all the persons named above my most cordial thanks are due for the zeal and steadiness with which they followed out my plans under the most distressing circumstances of weather. But to Captain SHIRREFF in particular an acknowledgement of my obligations must here be given. Not only was the assistance of men and materials from the Dock Yard furnished by Captain SHIRREFF in the way which I thought most desirable, but by his presence and by the interest in the operations which he displayed, the services of all the subordinate persons were rendered fully efficient; while the part which he took as an active observer, from the beginning to the end, materially lightened my labours and increased my confidence in the results.

Section II.—*Immediate Results of Observations.*

I. Local disturbances of the compass, as shown by the observations on the Raft.

A plan of the Basin in Deptford Dock Yard was prepared, from simultaneous observations with the two theodolites A. and B., on a signal carried to different parts of the basin-wall, and was copied (by pricking off) for the insertion of the places of the compass in every series of observations. The positions of the compass when on the raft were laid down on one of these copies, from simultaneous observations with the theodolites, as already described. The positions from No. 105 to 114 range along the E. and S.E. side of the basin, from the entrance to the S. angle: 115, 116, and 117, are on the S.W. side between the ways of two building slips; and the remainder, from 118 to 129, are on the N.W. side, from the W. angle to the entrance. The distance of the compass from the wharf varied from 16 to 24 feet. At each of these positions the chimney was observed with the compass. The next step (as in all the other observations) was to correct for locality, and this was done in the following manner. The angle at A, the south-eastern theodolite, between the chimney and the theodolite B. was 59° 29′, and the angle at B. between the chimney and A. was 113° 38′. Consequently the angle subtended by A B at the chimney was 6° 53′. The line A B being, therefore, supposed to represent 6° 53′, and being divided into portions corresponding each to 1°, commencing at B, dotted lines were drawn from the chimney through the points forming these divisions, and thus indicated the points in the basin at which the true azimuth of the chimney differs 1°, 2°, 3°, &c., from the azimuth observed at B. If then the correction + 1° be applied to the azimuths (reckoned from north through east, south, and west), as observed with the compass at any point in the dotted line marked + 1°, an azimuth will be obtained which ought to be the same as if it had been observed at B, and whose difference from that at B can be occasioned only by local disturbance; and similarly for positions of the compass upon any other dotted lines. For intermediate positions the correction was taken to 5′ by inspection. In this manner the following Table was formed.

No. of observation.	Observed azimuth reduced to B.	Mean of all the observed azimuths reduced.	Apparent local disturbance.	No. of observation.	Observed azimuth reduced to B.	Mean of all the observed azimuths reduced.	Apparent local disturbance.
105	37° 25′	37° 46′	−0° 21′	118	38° 45′	37° 46′	+0° 59′
106	37 10		−0 36	119	38 35		+0 49
107	38 5		+0 19	120	38 10		+0 24
108	36 30		−1 16	121	38 10		+0 24
109	37 30		−0 16	122	37 0		−0 46
110	37 45		−0 1	123	38 5		+0 19
111	37 45		−0 1	124	38 0		+0 14
112	37 40		−0 6	125	38 25		+0 39
113	37 45		−0 1	126	37 35		−0 11
114	37 40		−0 6	127	35 55		−1 51
115	38 5		+0 19	128	39 0		+1 14
116	38 15		+0 29	129	37 5		−0 41
117	37 55		+0 9				

The magnitude of these disturbances is so small as to leave it almost doubtful whether any correction ought to be applied for them. There is evidently no need for correction when the place of the compass is far removed from the side of the basin, as it was in all positions of the compass except at Station I. The only instances of the observations at Station I. in which corrections for local disturbance are applied are the following:

Nos. 1, 2, 3, 4, 6, 8, (nearly in the position of 105) + 0° 20′ each.
No. 24 (near the place of 119). . . . . − 0° 50′ each.
No. 25 (between 120 and 121). . . . . − 0° 25′ each.
No. 33 (near 106) . . . . + 0° 30′ each.

II. Azimuths of the ship's head and disturbance of the compass on the lower stage floor at Station I.

The chimney having been observed with the telescope carried by the spindle mounted on the compass, the reading of the compass card was at first expressed in the azimuths or amplitudes used by nautical men. This was then converted into azimuth reckoned from the N. through the E., S., and W., to N. proceeding from 0° to 360°. Then the correction for index error of the telescope was applied: then the correction for locality, and then (if necessary) the correction for local disturbance. The azimuth thus corrected would have been 37° 46′, had the ship caused no disturbance. The excess of the corrected azimuth above 37° 46′ is set down as the disturbance of the compass by the ship; the sign + denotes that the azimuth appears too great, or that the needle is deflected to the left.

The funnel of the steam-ship having been observed in the same way, its apparent azimuth, as affected by the ship's disturbance of the compass, was found in the same manner (omitting the correction for locality): correcting this for the disturbance (already found), the true azimuth was obtained.

In this manner the following table was formed.

No. of observation.	True azimuth of ship's head.	Disturbance of compass.	No. of observation.	True azimuth of ship's head.	Disturbance of compass.
1	203° 5′	−16° 50′	19	1° 35′	+40° 45′
2	214 53	−22 45	20	19 25	+51 20
3	220 0	−23 20	21	38 25	+50 50
4	230 0	−30 5	22	57 45	+46 30
5	240 50	−34 5	23	76 55	+39 15
6	248 50	−36 40	24	93 10	+33 5
8	256 4	−40 2	25	109 0	+26 40
9	267 44	−44 27	26	125 50	+19 40
10	276 12	−47 50	27	142 10	+11 55
11	282 57	−49 27	28	159 10	+ 4 20
12	292 17	−52 25	29	171 5	− 1 35
13	294 0	−53 45	30	189 45	−11 0
14	300 35	−54 50	31	202 5	−16 45
15	308 0	−53 50	32	220 25	−25 10
16	313 47	−54 2	33	248 55	−37 50
17	321 40	−50 10	34	278 15	−48 25
18	338 25	−33 0			

No. 7. was inadvertently omitted in the numeration. It will be remarked that the general positions of the vessel's head from No. 1 to 11 coincide nearly with those from No. 30 to 34. The following points are worthy of attention; 1st, that between Nos. 18 and 19, upon changing the position of the vessel's head 23°, the disturbance was altered 74°, so that the change of the vessel's position appeared on the compass card to be nearly 97°; 2nd, that the maximum of the positive errors is distinctly less than the maximum of the negative errors. The latter remark will be found to bear in an important degree on the theoretical explanation of the errors.

III. Disturbance of the compass on the lower stage floor at Station II.

The operation is in every respect the same as the last, except that no correction was applied for local disturbance.

No. of observation.	True azimuth of ship's head.	Disturbance of compass.	No. of observation.	True azimuth of ship's head.	Disturbance of compass.
35	282° 51′	−21° 36′	45	103° 36′	+12° 54′
36	300 4	−12 41	46	124 26	+ 7 26
37	320 49	− 1 19	47	143 57	+ 1 26
38	340 7	+ 9 31	48	163 16	− 4 16
39	353 48	+15 4	49	179 6	− 9 6
40	11 26	+18 34	50	203 1	−16 16
41	31 14	+19 46	51	222 21	−21 51
42	49 47	+19 51	52	244 56	−26 26
43	67 3	+18 12	53	267 46	−24 16
44	85 1	+14 59			

IV. Disturbance of the compass on the lower stage-floor at Station II., when the steam was up and the boilers and engine hot.

No. of observation.	True azimuth of ship's head.	Disturbance of compass.	No. of observation.	True azimuth of ship's head.	Disturbance of compass.
90	279° 1′	−21° 31′	98	99° 11′	+15° 19′
91	296 36	−13 6	99	115 26	+11 4
92	315 54	− 4 54	100	143 26	+ 3 4
93	329 6	+ 3 54	101	174 21	− 6 21
94	354 26	+12 34	102	218 26	−20 26
95	14 36	+16 54	103	246 41	−23 11
96	37 41	+19 49	104	275 1	−26 1
97	62 26	+18 34			

If these disturbances and those of the last Table be constructed graphically, the azimuth being taken as abscissa, and the disturbance of the compass as ordinate, and if a curve in each case be drawn through the points, it will be found that the curves do not sensibly differ. It appears therefore that no sensible part of the disturbance depends on the state of heat of the engines.

V. Disturbance of the Compass on the Lower Stage Floor at Station III.

No. of observation.	True azimuth of ship's head.	Disturbance of compass.	No. of observation.	True azimuth of ship's head.	Disturbance of compass.
54	(274° 11′)	− 19° 41′	64	93° 11′	+ 11° 49′
55	(310 24)	− 13 24	65	120 36	+ 7 54
56	311 18	− 4 18	66	139 46	+ 3 44
57	324 26	+ 2 49	67	163 9	− 3 9
58	340 51	+ 7 39	68	187 9	− 10 39
59	2 34	+ 11 56	69	216 6	− 18 21
60	22 16	+ 14 29	70	235 11	− 21 41
61	38 11	+ 16 19	71	259 59	− 24 29
62	60 34	+ 15 26	72	275 41	− 21 41
63	81 36	+ 13 24			

It is conjectured that the azimuth in No. 54 ought to be increased 10°, and that in No. 55 ought to be diminished 10°; and the subsequent calculations are made on this supposition.

VI. Disturbance of the Compass on the Lower Stage Floor at Station IV.

No. of observation.	True azimuth of ship's head.	Disturbance of compass.	No. of observation.	True azimuth of ship's head.	Disturbance of compass.
73	276° 21′	− 9° 21′	82	95° 6′	+ 7° 9′
74	291 6	− 2 6	83	130 6	+ 3 54
75	321 6	+ 0 24	84	146 46	− 0 46
76	330 36	+ 8 54	85	170 16	− 6 16
77	347 6	+ 8 54	86	198 1	− 12 31
78	3 41	+ 9 49	87	219 36	− 15 21
79	16 46	+ 10 14	88	245 9	− 15 39
80	49 54	+ 7 36	89	264 34	− 12 4
81	66 59	+ 8 1			

The azimuth or the disturbance in No. 75 appears to be wrong. It is not used in the subsequent calculations.

(To avoid confusion, I shall defer to a subsequent part of this paper the account of the disturbance of the compass on the upper stage floor.)

VII. Times of Vibration of a Needle suspended by a Silk Fibre near the Level of the Lower Stage Floor.

	s
On shore 30 vibrations were performed in	140·6
At Station I., the ship's head being N., the time of 30 vibrations was .	300·5
the ship's head being E., the time of 30 vibrations was .	119·5
the ship's head being S., the time of 30 vibrations was .	103·6
the ship's head being W., the time of 30 vibrations was .	133·9
At Station II., the ship's head being N., the time of 30 vibrations was .	166·3
the ship's head being E., the time of 30 vibrations was .	129·7
the ship's head being S., the time of 30 vibrations was .	120·8
the ship's head being W., the time of 30 vibrations was .	157·3
At Station III., the ship's head being N., the time of 30 vibrations was .	153·4
the ship's head being E., the time of 30 vibrations was .	130·2
the ship's head being S., the time of 30 vibrations was .	121·7
the ship's head being W., the time of 30 vibrations was .	158·5
At Station IV., the ship's head being N., the time of 30 vibrations was .	147·8
the ship's head being E., the time of 30 vibrations was .	130·7
the ship's head being S., the time of 30 vibrations was .	128·8
the ship's head being W., the time of 30 vibrations was .	172·3

VIII. Observations with Dipping Needles on board the Ship, near the Level of the Stage Floor at Stations I., III., IV., the Needles vibrating in the Vertical Plane passing through the Ship's Keel.

(The dipping-needle employed at Station I. is a small needle of very fine workmanship, by **Dollond**, lent by Mr. **Dollond** for the experiments. Its dip on shore appeared to be 72° 40′.

The dipping-needle used at Station III. is a larger needle, by **Jones**, lent by Mr. **Christie**. Its dip on shore was about 69° 10′.

The dipping-needle employed at Station IV. is an excellent needle belonging to the Admiralty, made by **Dollond**, of intermediate dimensions. Its dip on shore was about 69° 35′.)

The letter H denotes that the dip was towards the head, S that it was towards the stern. When the number of degrees exceeds 90° the needle is dipping on the side of the vertical, opposite to that denoted by a number of degrees less than 90.

No. of observation.	Azimuth of ship's head.	Dip at Station I.	Dip at Station III.	Dip at Station IV.
35	282° 51′	78° 50′ S.	89° 35′ H.	89° 20′ H.
36	300 4	....	83 21	79 55
37	320 49	87 20 S.	78 0	78 42
38	340 7	89 25 H.	72 6	69 10
39	353 48	88 10	71 37	68 10
40	11 26	88 5 H.	71 55	67 12
41	31 14	89 10 S.	74 47	68 32
42	49 47	86 10	79 45	65 17
43	67 3	82 30	84 59	85 35 H.
44	85 1	78 20	92 18 H.	88 20 S.
45	103 36	72 15	79 3 S.	78 50
46	124 26	68 0	71 53	67 0
47	143 57	64 55	66 58	61 30
48	163 16	61 50	63 28	58 25
49	179 6	61 5	62 28	58 10
50	203 1	61 50	63 55	58 30
51	222 21	64 40	68 40	68 22
52	244 56	68 35	75 10	69 5
53	267 46	74 5 S.	83 43 S.	79 32 S.

IX. Observations with dipping-needles near the level of the stage-floor at Stations I., III., IV.; the needles vibrating in the vertical plane transverse to the ship's keel.

(The needles are the same as those used in the last experiments.) The letter S. denotes that the dip was to the starboard, L. that it was to the larboard.

No. of observation.	Azimuth of ship's head.	Dip at Station I.	Dip at Station III.	Dip at Station IV.
54	284° 11′	78° 30′ S.	74° 45′ S.	70° 2′ S.
55	300 24	79 25	76 16	70 32
56	311 18	83 30	78 15	77 25
57	324 26	85 30	81 30 S.	80 58
58	340 51	89 5 S.	.. ..	87 58 S.
59	2 34	86 0 L.	.. ..	82 15 L.
60	22 16	80 0	79 16 L.	69 38
61	38 11	76 15	73 26	68 57
62	60 34	72 40	68 8	61 40
63	81 36	69 10	66 29	60 45
64	93 11	71 0	66 15	60 42
65	120 36	72 30	68 54	62 25
66	139 46	76 0	73 56	63 40
67	163 9	78 55	79 30	72 52
68	187 9	87 40 L.	89 3 L.	89 50 L.
69	216 6	85 0 S.	82 12 S.	83 45 S.
70	235 11	81 35	77 50	73 25
71	259 59	78 55	74 32	69 40
72	275 41	78 30 S.	74 40 S.	69 40 S.

(The observations with the dipping-needle and large magnets on shore will be deferred to another part of this paper.)

Section III.—*Theory of Induced Magnetism.*

The fundamental supposition of the theory on which I shall found the calculations in the following pages is, that, by the action of terrestrial magnetism, every particle of iron is converted into a magnet whose direction is parallel to that of the dipping-needle, and whose intensity is proportional to the intensity of terrestrial magnetism: the upper end having the property of attracting the north end of the needle, and the lower end that of repelling it.

It would have been desirable to make the calculations on Poisson's theory, which undoubtedly possesses greater claims on our attention, as a theory representing accurately the facts of some very peculiar cases, than any other. The difficulties, however, in the application of that theory to complicated cases, are great, perhaps insuperable. And in ordinary cases, the simpler theory that I have mentioned will give the same comparative though not the same absolute results. For instance, Poisson's theory explains the near equality between the attraction of a sphere or thick shell and that of a thin shell: the simpler theory does not explain this near equality; but the shell being given, it enables us to compute its comparative effects in different positions as well as POISSON'S. In the case of a spheroidal mass of iron, the result is similar. And as far as giving a general agreement between the results of calculation and the comparative effects of given masses in different positions, the theory appears sufficiently accurate.

Let the place of the compass be the origin of co-ordinates: let A be the azimuth of the ship's head, measured from the magnetic north towards the east: a the azimuth of any particle measured from the ship's head; so that $\mathrm{A} + a$ is the azimuth of that particle from the north. Let b be the angular depression of the particle. Then if r be the distance of the particle from the compass; x, y, z, the ordinates towards the north, towards the east, and vertically downwards; we shall have

$$x = r \,.\, \cos b \,.\, \cos \overline{\mathrm{A} + a}, \qquad y = r \,.\, \cos b \,.\, \sin \overline{\mathrm{A} + a}, \qquad z = r \,.\, \sin b.$$

Let I represent the intensity of terrestrial magnetism, δ the dip, m a constant depending on the mass of the particle, $2\,l$ the length of the small magnet into which it is changed. Then the ordinates of that end of the small magnet which attracts the north end of the needle will be

$$x - l \cos\delta, \qquad y, \qquad z \quad l \sin\delta.$$

Its distance (omitting the squares, &c. of l) $= \sqrt{\{r^2 - 2\,l\,x\cos\delta - 2\,l\,z\sin\delta\}}$

$$= r - \frac{l}{r}(x\cos\delta + z\sin\delta).$$

And the resolved parts of its attraction (supposing the whole attraction inversely as the n^{th} power of the distance) will be—In the direction of x,

$$\mathrm{I}\,m\,\frac{x - l\cos\delta}{\left\{r - \frac{l}{r}(x\cos\delta + z\sin\delta)\right\}^{n+1}}$$

$$= \mathrm{I}\,m \,.\, \frac{x}{r^{n+1}}\left\{1 - l \,.\, \frac{\cos\delta}{x} + l \,.\, \overline{n+1} \,.\, \frac{x\cos\delta + z\sin\delta}{r^2}\right\}.$$

In the direction of y,

$$\mathrm{I}\,m\,\frac{y}{\left\{r - \frac{l}{r}(x\cos\delta + z\sin\delta)\right\}^{n+1}}$$

$$= \mathrm{I}\,m\,\frac{y}{r^{n+1}}\left\{1 + l \,.\, \overline{n+1} \,.\, \frac{x\cos\delta + z\sin\delta}{r^2}\right\}.$$

In the direction of z,

$$\mathrm{I}\,m\,\frac{z - l\sin\delta}{\left\{r - \frac{l}{r}(x\cos\delta + z\sin\delta)\right\}^{n+1}}$$

$$= \mathrm{I}\,m \,.\, \frac{z}{r^{n+1}}\left\{1 - l \,.\, \frac{\sin\delta}{z} + l \,.\, \overline{n+1} \,.\, \frac{x\cos\delta + z\sin\delta}{r^2}\right\}.$$

In like manner, the repulsive force produced by the other end of the small magnet will be—In the direction of x

$$= \mathrm{I}\, m \frac{x}{r^{n+1}} \left\{ 1 + l . \frac{\cos\delta}{x} - l . \overline{n+1} . \frac{x\cos\delta + z\sin\delta}{r^2} \right\}.$$

In the direction of y

$$= \mathrm{I}\, m \frac{y}{r^{n+1}} \left\{ 1 - l . \overline{n+1} . \frac{x\cos\delta + z\sin\delta}{r^2} \right\}.$$

In the direction of z

$$= \mathrm{I}\, m . \frac{z}{r^{n+1}} \left\{ 1 + l . \frac{\sin\delta}{z} - l . \overline{n+1} . \frac{x\cos\delta + z\sin\delta}{r^2} \right\}.$$

The difference between the attractive and repulsive forces, or the true disturbing force, will be—In the direction of x,

$$- \mathrm{I}\cos\delta . \frac{2\, l m}{r^{n+1}} + \mathrm{I}\cos\delta . \frac{2 . \overline{n+1} . l m . x^2}{r^{n+3}} + \mathrm{I}\sin\delta . \frac{2 . \overline{n+1} . l m . x z}{r^{n+3}}.$$

In the direction of y,

$$\mathrm{I}\cos\delta . \frac{2 . \overline{n+1} . l m . x y}{r^{n+3}} + \mathrm{I}\sin\delta . \frac{2 . \overline{n+1} . l m . y z}{r^{n+3}}.$$

In the direction of z,

$$- \mathrm{I}\sin\delta . \frac{2\, l m}{r^{n+1}} + \mathrm{I}\cos\delta . \frac{2 . \overline{n+1} . l m . x z}{r^{n+3}} + \mathrm{I}\sin\delta . \frac{2 . \overline{n+1} . l m . z^2}{r^{n+3}}.$$

These are expressions for the disturbing forces produced by a single particle. To find the disturbing forces produced by the whole of the iron in the ship, we must take the sum of these expressions for every particle in the ship. Expressing this summation by the letter S, we have the whole disturbing forces as follows:

In the direction of x,

$$- \mathrm{I}\cos\delta . \mathrm{S}\frac{2\, l m}{r^{n+1}} + \mathrm{I}\cos\delta . \mathrm{S}\frac{2 . \overline{n+1} . l m . x^2}{r^{n+3}} + \mathrm{I}\sin\delta . \mathrm{S}\frac{2 . \overline{n+1} . l m . x z}{r^{n+3}}.$$

In the direction of y,

$$\mathrm{I}\cos\delta . \mathrm{S}\frac{2 . \overline{n+1} . l m . x y}{r^{n+3}} + \mathrm{I}\sin\delta . \mathrm{S}\frac{2 . \overline{n+1} . l m . y z}{r^{n+3}}.$$

In the direction of z,

$$- \mathrm{I}\sin\delta . \mathrm{S}\frac{2\, l m}{r^{n+1}} + \mathrm{I}\cos\delta . \mathrm{S}\frac{2 . \overline{n+1} . l m . x z}{r^{n+3}} + \mathrm{I}\sin\delta . \mathrm{S}\frac{2 . n \overline{+ 1} . l m . z^2}{r^{n+3}}.$$

We will now transform these formulæ by substituting for x, y, and z, their values in terms of r, A, a, and b. And we will suppose the compass to be in the vertical plane passing through the ship's keel, and the arrangement of the iron on both sides of that plane to be symmetrical. Then

$$\frac{x^2}{r^{n+3}} = \frac{\cos^2 b . \cos^2 \overline{\mathrm{A} + a}}{r^{n+1}} = \frac{1}{2}\frac{\cos^2 b}{r^{n+1}} + \frac{1}{2}\frac{\cos^2 b}{r^{n+1}}(\cos 2\,\mathrm{A} . \cos 2\,a - \sin 2\,\mathrm{A} . \sin 2\,a).$$

But

$$S\,\frac{l\,m\cos^2 b\,.\,\sin 2\,A\,.\,\sin 2\,a}{r^{n+1}},\ \text{or}\ \sin 2\,A\,.\,S\,\frac{l\,m\cos^2 b\,.\,\sin 2\,a}{r^{n+1}},$$

will evidently = 0, because for the same value of l, m, b, and r, there will be two values of a, one positive and the other negative. Hence the term

$$S\,\frac{2\,.\,\overline{n+1}\,.\,l\,m\,.\,x^2}{r^{n+3}}$$

will become

$$\overline{n+1}\,.\,S\,\frac{l\,m\,.\,\cos^2 b}{r^{n+1}} + \cos 2\,A\,.\,\overline{n+1}\,.\,S\,\frac{l\,m\,.\,\cos^2 b\,.\,\cos 2\,a}{r^{n+1}}.$$

The value of $\frac{x\,z}{r^{n+3}}$ is

$$\frac{\sin b\,.\,\cos b\,.\,(\cos A\,.\,\cos a - \sin A\,.\,\sin a)}{r^{n+1}}$$

in which, for the same reason, the term $\sin A\,.\,\sin a$ is to be rejected. Hence the term

$$S\,\frac{2\,.\,\overline{n+1}\,.\,l\,m\,.\,x\,z}{r^{n+3}}$$

will become

$$\cos A\,.\,2\,.\,\overline{n+1}\,.\,S\,\frac{l\,m\,.\,\sin b\,.\,\cos b\,.\,\cos a}{r^{n+1}}.$$

The value of $\frac{x\,y}{r^{n+3}}$ is

$$\frac{1}{2}\,\frac{\cos^2 b\,(\sin 2\,A\,.\,\cos 2\,a + \cos 2\,A\,.\,\sin 2\,a)}{r^{n+1}}.$$

Hence the term

$$S\,\frac{2\,.\,\overline{n+1}\,.\,l\,m\,.\,x\,y}{r^{n+3}}$$

will become

$$\sin 2\,A\,.\,\overline{n+1}\,.\,S\,\frac{l\,m\,.\,\cos^2 b\,.\,\cos 2\,a}{r^{n+1}}.$$

The value of $\frac{y\,z}{r^{n+3}}$ is

$$\frac{\sin b\,.\,\cos b\,(\sin A\,.\,\cos a + \cos A\,.\,\sin a)}{r^{n+1}}.$$

Hence the term

$$S\,\frac{2\,.\,\overline{n+1}\,.\,l\,m\,.\,y\,z}{r^{n+3}}$$

will become

$$\sin A\,.\,2\,.\,\overline{n+1}\,.\,S\,\frac{l\,m\,.\,\sin b\,.\,\cos b\,.\,\cos a}{r^{n+1}}.$$

The value of $\frac{z^2}{r^{n+3}}$ is $\frac{\sin^2 b}{r^{n+1}}$, which admits of no further reduction.

Hence the expressions for the whole disturbing forces are,—In the direction of x,

$$-\mathrm{I}\cos\delta\left\{\mathrm{S}\,\frac{2\,l\,m}{r^{n+1}}-\overline{n+1}\,.\,\mathrm{S}\,\frac{l\,m\,.\cos^2 b}{r^{n+1}}\right\}+\mathrm{I}\cos\delta\,.\cos 2\,\mathrm{A}\,.\,\overline{n+1}\,.\,\mathrm{S}\,\frac{l\,m\cos^2 b\,.\cos 2\,a}{r^{n+1}}$$

$$+\,\mathrm{I}\sin\delta\,.\,\cos\mathrm{A}\,.\,2\,.\,\overline{n+1}\,.\,\mathrm{S}\,\frac{l\,m\,.\sin b\,.\cos b\,.\cos a}{r^{n+1}}.$$

In the direction of y,

$$\mathrm{I}\cos\delta\,.\sin 2\,\mathrm{A}\,.\,\overline{n+1}\,.\,\mathrm{S}\,\frac{l\,m\,.\cos^2 b\,.\cos 2\,a}{r^{n+1}}+\mathrm{I}\sin\delta\,.\sin\mathrm{A}\,.\,2\,.\,\overline{n+1}\,.\,\mathrm{S}\,\frac{l\,m.\sin b\,.\cos b.\cos a}{r^{n+1}}$$

In the direction of z,

$$-\mathrm{I}\sin\delta.\left\{\mathrm{S}\,\frac{2\,l\,m}{r^{n+1}}-2\,.\,\overline{n+1}\,.\,\mathrm{S}\,\frac{l\,m\,.\sin^2 b}{r^{n+1}}\right\}+\mathrm{I}\cos\delta\,.\cos\mathrm{A}.2.\overline{n+1}\,.\,\mathrm{S}\,\frac{l\,m.\sin b.\cos b.\cos a}{r^{n+1}}.$$

Let

$$\mathrm{S}\,\frac{2\,l\,m}{r^{n+1}}-\overline{n+1}\,.\,\mathrm{S}\,\frac{l\,m\,.\cos^2 b}{r^{n+1}}=\mathrm{M}$$

$$2\,.\,\overline{n+1}\,.\,\mathrm{S}\,\frac{l\,m\,.\sin b\,.\cos b\,.\cos a}{r^{n+1}}=\mathrm{N}$$

$$\overline{n+1}\,.\,\mathrm{S}\,\frac{l\,m\,.\cos^2 b\,.\cos 2\,a}{r^{n+1}}=\mathrm{P}$$

$$\mathrm{S}\,\frac{2\,l\,m}{r^{n+1}}-2\,.\,\overline{n+1}\,.\,\mathrm{S}\,\frac{l\,m\,.\sin^2 b}{r^{n+1}}=\mathrm{Q}.$$

The four quantities M, N, P, Q, are then constant, depending solely upon the construction of the ship, not changing with any variations of terrestrial locality, or of magnetic dip or intensity. Then the disturbing forces (always estimated by their action on the north or marked end of the needle) are

Towards the magnetic north $-\mathrm{I}\cos\delta\,.\,\mathrm{M}+\mathrm{I}\cos\delta\,.\,\mathrm{P}\,.\cos 2\,\mathrm{A}+\mathrm{I}\sin\delta\,.\,\mathrm{N}\,.\cos\mathrm{A}$.

Towards the magnetic east . $\mathrm{I}\cos\delta\,.\,\mathrm{P}\,.\sin 2\,\mathrm{A}+\mathrm{I}\sin\delta\,.\,\mathrm{N}\,.\sin\mathrm{A}$.

Vertically downwards . . . $-\mathrm{I}\sin\delta\,.\,\mathrm{Q}+\mathrm{I}\cos\delta\,.\,\mathrm{N}\,.\cos\mathrm{A}$.

Before transforming these into another shape, we will consider the construction which they indicate as proper for the correction of a compass disturbed by the induced magnetism only of the iron in a ship.

The only force which it is necessary to destroy is that directed to the east, or

$$\mathrm{I}\cos\delta\,.\,\mathrm{P}\,.\sin 2\,\mathrm{A}+\mathrm{I}\sin\delta\,.\,\mathrm{N}\,.\sin\mathrm{A}.$$

In the usual cases (at least for wood-built ships) P and N will both have positive values; the iron being supposed to lie almost entirely on one side of the compass (so that $\cos a$ and $\cos 2\,a$ are almost always positive), and being almost entirely at a lower level than the compass (so that $\sin b\cos b$ is almost always positive).

Suppose now we find a mass of iron, which when placed at a certain distance and a certain depression, and in the same azimuth as the ship's head, will produce the same disturbance of the compass. (This is the method of determining the distance, &c. for BARLOW's plate.) Then this mass, in the azimuth A, produces the disturbing force to the east, $I \cos \delta . P . \sin 2 A + I \sin \delta . N . \sin A$; and therefore in the azimuth A' it produces the disturbing force to the east, $I \cos \delta . P . \sin 2 A' + I \sin \delta . N . \sin A'$. Suppose now that the mass is placed towards the stern of the ship (or on the side opposite to that in which the ship's iron is, for the most part, situated), but at the same distance and depression as those already determined. (This is the rule for applying Barlow's plate as a partial corrector.) A' must now be made $= A + 180°$; and the disturbing force produced by the mass is now $I \cos \delta . P . \sin 2 A - I \sin \delta . N . \sin A$. Combining this with the disturbing force produced by the ship's iron, the compound force is reduced to $2 I \cos \delta . P . \sin 2 A$; that is, one of the terms expressing the disturbing force is destroyed, and the other is doubled. Whether this change of the force is advantageous or prejudicial, will depend not only on the value of δ (the dip), but also on the proportion between the values of N and P (that is, on the way in which the iron is distributed with regard to elevation above or depression below the compass, and with regard to the manner in which it surrounds the compass).

Let us consider, for instance, the case of a long wood-built steam-boat, with a compass on deck or in a cabin. It is probable that the iron of the engines and funnel may be pretty equally distributed above and below the level of the compass. Here $\sin b . \cos b$ has as many negative as positive values, and therefore $N = 0$. But $\cos 2 a$ is always positive, because the azimuth of any part of the iron from the vertical plane passing through the keel does not amount to 45°, and therefore P is positive. Consequently in this instance the application of Barlow's plate doubles the error of the compass.

Yet it is possible in any case to destroy the disturbing force entirely. Suppose that Barlow's plate is fixed as above mentioned, and that the disturbing force is therefore $2 I \cos \delta . P . \sin 2 A$. Now let a second mass of iron be introduced, with its centre at the same level as the compass, and in the azimuth A''. The general expression for the force which it produces is

$$I \cos \delta . P'' . \sin 2 A'' + I \sin \delta . N'' . \sin A''.$$

But $N'' = 0$ (because there are as many negative values of $\sin b . \cos b$ as there are positive). Combining this, then, with the force produced by the ship's iron and by the Barlow's plate already mounted, the whole force is

$$I \cos \delta . (2 P . \sin 2 A + P'' . \sin 2 A'').$$

Now this quantity may be made $= 0$ by making $A'' = A \pm 90°$, and $P'' = 2 P$; for then

$$\sin 2 A'' = \sin (2 A \pm 180°) = - \sin 2 A,$$

and the second factor becomes

$$2 P . \sin 2 A - 2 P . \sin 2 A.$$

Hence we get the following simple rule for the perfect correction of the compass.

1. Determine the position of Barlow's plate, with regard to the compass, which

will produce the same effect as the iron in the ship. (It will be sufficient if when placed on the E. side it produces the same effect as the ship when the ship's head is E.)

2. Fix Barlow's plate at the distance and depression determined by the last experiment, but in the opposite azimuth (or towards the ship's stern).

3. Mount another mass of iron at the same level as the compass, but on the starboard or larboard side, and determine its position so that the compass shall point correctly when the ship's head is N.E., S.E., S.W., or N.W.

Then the compass will be correct in all positions of the ship's head, and in all magnetic latitudes.

When the disturbing iron of the ship is at the same level as the compass, the correction is much more simple. It is only necessary then to introduce a single mass of iron at the starboard or larboard side, and at the same level as the compass. For the ship's disturbing force is then

$$I \cos \delta \,.\, P \,.\, \sin 2 A,$$

and the force produced by this mass is

$$I \cos \delta \,.\, P'' \,.\, \sin 2 A'';$$

and if $A'' = A \pm 90°$, the sum of these terms will be 0, provided $P'' = P$, the distance for which condition will be easily ascertained by experiment.

In general it may be remarked, that if one mass of iron is placed exactly opposite another equal mass, both in azimuth and in elevation, it doubles its disturbing effect; if one mass be placed opposite the other in azimuth and at the same elevation or depression, or if it be placed in the same azimuth but with elevation instead of depression, or *vice versâ*, it destroys that term of the disturbance which depends on sin A, and doubles that which depends on sin 2 A. And if one mass be placed at the same level as the compass, its effects may be destroyed by placing another mass at the same level, in azimuth differing 90° on either side. (This conclusion, for spherical masses, was also obtained by Poisson.) To this we may add, that if a disturbance, from whatever cause arising, follows the law of $+ \sin 2 A$ (changing sign in the successive quadrants, and positive when the ship's head is between N. and E.), it may be destroyed by placing a mass of iron on the starboard or larboard side, at the same level as the compass; if it follows the law of $- \sin 2 A$, the mass of iron must be on the fore or aft side.

The form of Barlow's plate appears objectionable, in so far as having its broad side turned towards the compass, it occupies a considerable arc of azimuth, and $\cos 2 a$ may therefore (for some parts of it) be small or negative. A plate of iron rolled into a scroll, and placed with its end directed towards the compass, appears better; but I prefer a long box filled with iron chain, as less likely to possess the permanent magnetism, from which no plate iron is free; its end should be directed towards the compass.

I trust that in the preceding remarks on the imperfection of Barlow's plate as a corrector, I shall not appear to have treated Mr. Barlow's construction with harshness. I can truly assert that my feelings and my intentions are of a very different character. To Mr. Barlow we are indebted for almost all the experimental knowledge which we possess on the subject of the disturbance produced by masses of iron; the use of his plate *as a corrector* was avowedly proposed by him as imperfect; and it requires no great experience in the pursuit of experimental and practical philosophy to learn to venerate the man who makes the first step in devising a construction applicable to a given purpose. I must not omit to add, that Mr. Barlow's plate corrects that part of the disturbance which is most important in the most critical circumstances, namely, in the high magnetic latitudes. Without Mr. Barlow's proposed construction I should never have arrived at the more perfect construction described above; with it the invention of something more perfect was easy.

We will now resume the consideration of the expressions for the disturbing forces produced by the ship.

The first term in the expression for the disturbance towards the north is constant. It appears therefore that if M be positive, the absolute directive force on the needle will be, on the whole, diminished. On examining the expression for M it will be seen that its value is greatest (for a given mass of iron at a given distance) when the iron is immediately above or below the compass, and that it is least when the iron is at the same level as the compass. It is proper therefore that both in the construction of the ship and in the fixing of correctors, no large mass of iron should be placed below the compass.

The expression for disturbing force towards the ship's head is,

$$\cos A \times \text{force towards north} + \sin A \times \text{force towards east},$$

$$= I \cos \delta . (-M + P) \cos A + I \sin \delta . N.$$

The second term of this expression is independent of the position of the ship, and therefore cannot be distinguished, in experiments made at any one locality, from permanent magnetism. But as it contains the factor $I \sin \delta$, or the vertical force of terrestrial magnetism, it may be discovered from experiments made in different localities, where the magnitude of the vertical force differs much.

The expression for the disturbing force towards the starboard side of the ship is

$$\cos A \times \text{force towards east} - \sin A \times \text{force towards north},$$

$$= I \cos \delta . (M + P) \sin A.$$

It will be convenient to remember that M is the coefficient on which the absolute diminution of the directive force depends; that N is the coefficient upon which depends the force similar to permanent magnetism; and that P is the coefficient upon which depends the transversal force changing sign in the successive quadrants of azimuth.

It will also be convenient to remember that M is largest when the mass of iron is below, and may be negative if the mass of iron is nearly at the same level; that N will vanish if the mass of iron is nearly at the same level as the compass, or may vanish from the opposition of different masses in azimuth while their depression is similar; and that P will vanish if the mass of iron is below, or may vanish from the opposition of effect produced by masses in azimuths differing 90°, but that masses in opposite azimuths combine to increase P.

It may now be desirable to consider the modifications produced in the horizontal forces by the heeling of the ship. The pitching causes a variation in the position of the keel alternately in one direction and in the opposite, and its effects may therefore probably be neglected; but the heeling frequently continues in the same direction for many hours or days, and therefore it will not be safe to omit it. To simplify our expressions, we will suppose the angle of heel to be not very great.

Let the different particles of the ship be referred to the place of the compass by means of the angles a and b as before, but let a be the azimuth of any particle from the direction of the ship's keel as measured in a plane parallel to that of the deck, and let b be the angle of depression as measured, from the plane parallel to the deck, towards the line which is normal to the deck. Then a and b are independent of the ship's heeling. Let h be the angle of heel towards the starboard side; and suppose h so small that its square may be neglected. Then we have

$$x = r \, . \cos b \, . \cos a \, . \cos \mathrm{A} - r \, . \cos b \, . \sin a \, . \sin \mathrm{A} + \sin h \, . r \, . \sin b \, . \sin \mathrm{A}.$$

$$y = r \, . \cos b \, . \cos a \, . \sin \mathrm{A} + r \, . \cos b \, . \sin a \, . \cos \mathrm{A} - \sin h \, . r \, . \sin b \, . \cos \mathrm{A}.$$

$$z = r \, . \sin b + \sin h \, . r \, . \cos b \, . \sin a.$$

The new term introduced into x^2 is

$$\sin h \, . 2 r^2 \{\sin b \, . \cos b \, . \cos a \, . \sin \mathrm{A} \, . \cos \mathrm{A} - \sin b \, . \cos b \, . \sin a \, . \sin^2 \mathrm{A}\},$$

and therefore the new term introduced into

$$\mathrm{I} \cos \delta \, . \mathrm{S} \frac{2 \, . \overline{n+1} \, . l m \, . x^2}{r^{n+3}}$$

will be

$$\sin h \, . \mathrm{I} \cos \delta \, . \sin 2 \mathrm{A} \, . \mathrm{S} \frac{2 \, . \overline{n+1} \, . l m \, . \sin b \, . \cos b \, . \cos a}{r^{n+1}} = \sin h \, . \mathrm{I} \cos \delta \, . \mathrm{N} \, . \sin 2 \mathrm{A}.$$

The new term introduced into $x z$ will be

$$\sin h \, . r^2 \{\cos^2 b \, . \sin a \, . \cos a \, . \cos \mathrm{A} - \cos^2 b \, . \sin^2 a \, . \sin \mathrm{A} + \sin^2 b \, . \sin \mathrm{A}\},$$

and therefore the new term introduced into

$$\mathrm{I} \sin \delta \, . \mathrm{S} \frac{2 \, . \overline{n+1} \, . l m \, . x z}{r^{n+3}}$$

will be

$$\sin h \, . \mathrm{I} \sin \delta \, . \sin \mathrm{A} \, . \mathrm{S} \frac{2 \, . \overline{n+1} \, . l m \, . (\sin^2 b - \cos^2 b \, . \sin^2 a)}{r^{n+1}}.$$

Let

$$\mathrm{S} \frac{2 \, . \overline{n+1} \, . l m \, (\sin^2 b - \cos^2 b \, . \sin^2 a)}{r^{n+1}} = \mathrm{R};$$

(it is easily seen that $\mathrm{R} = \mathrm{M} + \mathrm{P} - \mathrm{Q}$); then the new term introduced into

$$\mathrm{I}\sin\delta\,.\,\mathrm{S}\,\frac{2\,.\,\overline{n+1}\,.\,lm\,.\,xz}{r^{n+3}} \text{ is}$$

$$\sin h\,.\,\mathrm{I}\sin\delta\,.\,\mathrm{R}\,.\,\sin\mathrm{A}.$$

Hence the whole addition to the force towards the magnetic north is

$$\sin h\,.\,\mathrm{I}\cos\delta\,.\,\mathrm{N}\,.\,\sin 2\,\mathrm{A} + \sin h\,.\,\mathrm{I}\sin\delta\,.\,\mathrm{R}\,.\,\sin\mathrm{A}.$$

The new term introduced into xy is

$$\sin h\,.\,r^2\,\{-\sin b\,.\,\cos b\,.\,\cos a\,(\cos^2\mathrm{A} - \sin^2\mathrm{A}) + 2\sin b\,.\,\cos b\,.\,\sin a\,.\,\sin\mathrm{A}\,.\,\cos\mathrm{A}\},$$

and therefore the new term introduced into

$$\mathrm{I}\cos\delta\,.\,\mathrm{S}\,\frac{2\,.\,\overline{n+1}\,.\,lm\,.\,xy}{r^{n+3}}$$

will be

$$-\sin h\,.\,\mathrm{I}\cos\delta\,.\,\cos 2\,\mathrm{A}\,.\,\mathrm{S}\,\frac{2\,.\,\overline{n+1}\,.\,lm\,.\,\sin b\,.\,\cos b\,.\,\cos a}{r^{n+1}} = -\sin h\,.\,\mathrm{I}\cos\delta\,.\,\mathrm{N}\,.\,\cos 2\,\mathrm{A}.$$

The new term introduced into yz is

$$\sin h\,.\,r^2\,\{\cos^2 b\,.\,\sin a\,.\,\cos a\,.\,\sin\mathrm{A} + \cos^2 b\,.\,\sin^2 a\,.\,\cos\mathrm{A} - \sin^2 b\,.\,\cos\mathrm{A}\},$$

and therefore the new term introduced into

$$\mathrm{I}\sin\delta\,.\,\mathrm{S}\,\frac{2\,.\,\overline{n+1}\,.\,lm\,.\,yz}{r^{n+3}}$$

will be

$$\sin h\,.\,\mathrm{I}\sin\delta\,.\,\cos\mathrm{A}\,.\,\mathrm{S}\,\frac{2\,.\,\overline{n+1}\,.\,lm\,.\,(\cos^2 b\,.\,\sin^2 a - \sin^2 b)}{r^{n+1}} = -\sin h\,.\,\mathrm{I}\sin\delta\,.\,\mathrm{R}\cos\mathrm{A}.$$

Hence the whole addition to the force towards the magnetic east is

$$-\sin h\,.\,\mathrm{I}\cos\delta\,.\,\mathrm{N}\,.\,\cos 2\,\mathrm{A} - \sin h\,.\,\mathrm{I}\sin\delta\,.\,\mathrm{R}\,.\,\cos\mathrm{A}.$$

Combining the expressions just found with those found in the first investigation of forces, we have the following:

Whole disturbing force to magnetic north $= -\mathrm{I}\cos\delta\,.\,\mathrm{M} + \mathrm{I}\cos\delta\,.\,\mathrm{P}\,.\,\cos 2\,\mathrm{A}$
$+ \mathrm{I}\sin\delta\,.\,\mathrm{N}\,.\,\cos\mathrm{A} + \sin h\,\{\mathrm{I}\cos\delta\,.\,\mathrm{N}\,.\,\sin 2\,\mathrm{A} + \mathrm{I}\sin\delta\,.\,\mathrm{R}\,.\,\sin\mathrm{A}\}$.

Whole disturbing force to magnetic east $= \mathrm{I}\cos\delta\,.\,\mathrm{P}\,.\,\sin 2\,\mathrm{A} + \mathrm{I}\sin\delta\,.\,\mathrm{N}\,.\,\sin\mathrm{A}$
$+ \sin h\,.\,\{-\mathrm{I}\cos\delta\,.\,\mathrm{N}\,.\,\cos 2\,\mathrm{A} - \mathrm{I}\sin\delta\,.\,\mathrm{R}\,.\,\cos\mathrm{A}\}$.

Transforming these into expressions for the forces in directions related to the direction of the ship's keel, we have

Whole disturbing force towards ship's head
$= -\mathrm{I}\cos\delta\,.\,\mathrm{M}\,.\,\cos\mathrm{A} + \mathrm{I}\cos\delta\,.\,\mathrm{P}\,.\,\cos\mathrm{A} + \mathrm{I}\sin\delta\,.\,\mathrm{N} + \sin h\,.\,\mathrm{I}\cos\delta\,.\,\mathrm{N}\,.\,\sin\,.\,\mathrm{A}$.

Whole disturbing force in the horizontal plane, towards the starboard side
$= \mathrm{I}\cos\delta\,.\,\mathrm{M}\,.\,\sin\mathrm{A} + \mathrm{I}\cos\delta\,.\,\mathrm{P}\,.\,\sin\mathrm{A} + \sin h\,\{-\mathrm{I}\cos\delta\,.\,\mathrm{N}\,.\,\cos\mathrm{A} - \mathrm{I}\sin\delta\,.\,\mathrm{R}\}$.

We have already shown how the terms independent of h (except those multiplied by M) may be neutralized. One part of the operation was, to place aft the compass a mass of iron which if in front of the compass would have given the same value of N which the ship gives. Now when this mass of iron is fixed aft, we may either regard it as an additional part of the ship in the same azimuth A and having the same angle of heel h, but giving a constant N negative in magnitude by reason of the factor $\cos a = \cos 180°$ which enters into its formation; or we may regard it as a new mass, with positive coefficient N, with azimuth $A + 180°$, with angle of heel $-h$, and whose action is estimated in the resulting expressions as towards the azimuth $A + 180°$ or from the ship's head. In either way of considering it, we find that by the process for neutralizing the term depending on N when the ship is not heeling, we have also neutralized the terms depending on N when the ship is heeling: and these terms may at once be put out of consideration. The only term, therefore, which remains to be considered is the following:

Disturbing force in the horizontal plane towards the starboard side, depending on the ship's heeling, $= -\sin h . \mathrm{I} \sin \delta . \mathrm{R}$.

For the *general* correction of this term, no easy method suggests itself. But the following considerations may serve to show the probability that constructions already proposed for another purpose will in a great measure correct it. First, with regard to the order of magnitude of this term; as $\mathrm{R} = \mathrm{M} + \mathrm{P} - \mathrm{Q}$, it is a term of the same order as the others which occur in this investigation (before multiplication by the factor $\sin h$), and therefore such masses of iron as those which are competent to correct the other terms may be expected to be of sufficient magnitude, with proper arrangement, to correct this term. Next, with regard to its law as depending on the position of the particles of iron: R is positive if $\sin^2 b$ exceeds $\cos^2 b . \sin^2 a$: but $r . \sin b$ is the depression of any particle below the compass measured perpendicularly to the deck, and $r . \cos b . \sin a$ is the ordinate measured from the plane passing through the keel and masts towards the starboard side. Hence we find this rule. If a line parallel to the keel be drawn through the compass, and if two planes be drawn through this line, inclined at angles of 45° to the plane passing through the masts and keel, so that a transverse section of the ship at any place present the section X: then all the iron in the upper and lower angles tends to increase R, and all the iron in the starboard and larboard angles tends to diminish R.

The principal part of the ship's iron will probably be in the lower angle, and will, therefore, tend to make R positive. Now we have, for correction of the term P, proposed to place a mass of iron on the starboard or larboard side, at the same level as the compass. It is evident that this iron tends to make R negative, and, therefore, tends to correct the effect of the ship's iron in disturbing the compass while the ship is heeling. Whether by varying the position of the mass of iron used to neutralize N, and thereby varying the magnitude both of P and of R, it may be practicable to make this lateral mass of iron neutralize both P and R, I cannot say. If it be impracticable, the accurate neutralization of P is evidently to be secured in preference to that of R. In any case, however, it will be possible to neutralize R, or the remaining part of R, for any given magnetic latitude, by placing a magnet in a position perpendicular to the ship's deck, and with its centre at the same level as the centre of the compass. To secure rigorously the latter condition, the place of the magnet should be fore or aft the compass. If we suppose the marked end of the magnet to be downwards, its action downwards parallel to the mast may be represented by $-$ V′: and when the ship heels, its action to the starboard side will be V′ . sin h: and the whole disturbing force towards the starboard side will, therefore, be sin h (V′ $-$ I sin δ . R), which by proper determination of the magnet's distance (since V′ may be increased or diminished in almost any proportion) may be made $= 0$. If R or the remaining part of R is negative, the marked end of the magnet must be upwards. In consequence of the multiplication of one of the terms by sin δ, the correction thus made will be good for only one magnetic latitude.

The proportion which the term sin h . I sin δ . R bears to the directive force or I cos δ, being expressed by the fraction sin h . tan δ . R, it is evident that this term, in high magnetic latitudes, may become important. I would, therefore, recommend that, when opportunity serves, an attempt should be made to determine the position of a magnet which will neutralize R. This can be done, so far as I see, only by trial with the ship's masts considerably inclined. Such an operation would probably be too troublesome in icy seas: but it might be possible to make the necessary observations in England, and then (by trial of the powers of the magnet at different distances) to define the relation between the distance of the magnet and the dip of the dipping needle, so that, knowing the dip, the magnet could be placed at once in the position in which it will neutralize R. A graduated slider to carry the magnet would make the practical use of this method very simple.

Section IV.—*Combination of Induced Magnetism with Permanent Magnetism.*

It is supposed in the following investigations that the existence of permanent magnetism produces no modification in the induced magnetism, their effects being simply combined by algebraical addition.

Whatever be the number or direction of the magnets entering into the composition of the ship, their effects on the compass may be represented by three forces: namely, one directed to the ship's head, one directed to the starboard side, and one vertically downwards. We will designate these by the letters H, S, and V, respectively.

The resolved parts of the two horizontal forces in the north and east directions are respectively H cos A $-$ S sin A, and H sin A $+$ S cos A. If we combine these with the expressions found for the disturbance produced in the same directions by induced magnetism, we obtain expressions which are rather long and troublesome to use. It will be better to use the expressions for the disturbing forces directed to the head and to the starboard side. We have then

Whole disturbing force towards the ship's head

$$= H + I\cos\delta\,.\,(-M + P)\,.\cos A + I\sin\delta\,.\,N.$$

Whole disturbing force towards the starboard side

$$= S + I\cos\delta\,.\,(M + P)\,.\sin A.$$

We shall now point out the way in which the numerical values of these quantities may be found from experiment.

A needle, delicately suspended, being made to vibrate horizontally, and the time occupied by a certain number of vibrations being noted, in a place on shore free from local disturbance, and also in the place of the ship's compass in any position of the ship's head, the ratio of the whole intensity of the horizontal force in the place of the ship's compass to the intensity on shore ($I\cos\delta$) is known, being the inverse ratio of the squares of the times of vibration. This force, however, is not directed to the north, but is in the disturbed direction of the needle of the ship's compass. The amount of the needle's angular disturbance towards the east being found by observation, the intensity just found must be multiplied by the cosine of the angular disturbance to give the resolved part of the force directed to the north, and by the sine of the angular disturbance to give the resolved part of the force directed to the east. The former of these, diminished by the intensity on shore, gives the actual disturbing force to the north: the latter, unaltered, gives the actual disturbing force to the east. The disturbing forces to the head and to the starboard side may now be calculated numerically, by giving numerical values to every part of these formulæ:

Disturbing force to head

= cos A × disturbing force to north + sin A × disturbing force to east.

Disturbing force to starboard side

= cos A × disturbing force to east − sin A × disturbing force to north.

These calculations are to be made for every position of the ship in which the vibraions have been observed. When $A = 0°, 90°, 180°, 270°$, the factors are 0 or 1: when $A = 45°, 135°, 225°, 315°$, the factors are equal in magnitude: in other positions of the ship, the numerical expressions for the factors are not so simple.

The calculations being performed, we have now two series of numbers corresponding to a series of different values of A; the numbers of one series are to represent (errors of observation excepted) different values of

$$\left(\frac{H}{I\cos\delta} + \tan\delta\,.\,N\right) + (-M + P)\cos A,$$

and the numbers of the other series are to represent different values of

$$\frac{S}{I\cos\delta} + (M + P)\sin A$$

and from these conditions the several constants

$$\left(\frac{H}{I\cos\delta} + \tan\delta\,.\,N\right), \qquad (-M + P), \qquad \frac{S}{I\cos\delta}, \qquad \text{and } (M + P)$$

are to be determined. This may be done by any of the methods used in astronomical or physical inquiries for determining the numerical values of the constants in a given formula which will best satisfy a number of equations of condition. The values of the constants, so found, being substituted in each equation, the agreement of the result with the number deduced from observation, within the limits of errors of observation, may be considered as a general proof of the correctness of the theory and of the accuracy of the numerical operation.

One of the constants thus found is $\frac{H}{I\cos\delta} + \tan\delta \,.\, N$. We have no means of determining separately the parts of which it consists, by observations made at one place. In a wood-built ship, where the various irons are laid in all possible positions, and where a great proportion consists of cast-iron (which does not appear usually to possess permanent magnetism) it is probable that the term $\frac{H}{I\cos\delta}$ is insignificant. In an iron-built ship we can conjecture the value of N from the values found for M and P: it will appear probable, from the subsequent investigations, that N is small, while $\frac{H}{I\cos\delta}$ may be very large.

From the values of $(-M+P)$ and $(M+P)$, the values of M and P will be immediately obtained.

We will now proceed with the consideration of the means of correcting the compass, so that its needle shall always point truly north. A due consideration of the preceding theory will show that this may be effected by a magnet or combination of magnets, and by a mass of iron, at the same level as the compass, placed on the starboard or larboard side if P is positive, or on the fore or aft side if P is negative.

Let H' be the force of the correcting magnets directed towards the head; S' the force directed to the starboard side; A' the azimuth of the mass of iron; P' the coefficient for this mass corresponding to P for the ship; M' and N' will be $= 0$. The forces produced by the mass of iron will be

Towards the north . . . . $I\cos\delta \,.\, P' \,.\, \cos 2A'$.
Towards the east . . . . $I\cos\delta \,.\, P' \,.\, \sin 2A'$.

From which the following are obtained:

Force towards the head . . . $= I\cos\delta \,.\, P' \,.\, \cos(2A' - A)$.
Force towards the starboard side $= I\cos\delta \,.\, P' \,.\, \sin(2A' - A)$.

And if the mass be on the starboard or larboard side of the compass, $A' = A \pm 90°$, and the expressions become

Force towards the head . . . $= -I \,.\, \cos\delta \,.\, P' \,.\, \cos A$.
Force towards the starboard side $= -I \,.\, \cos\delta \,.\, P' \,.\, \sin A$.

Combining these forces and those produced by the correcting magnets with the forces produced by the induced and permanent magnetism of the ship, we have the following:

Disturbing force towards the ship's head

$$= (H + I \sin\delta . N + H') + I\cos\delta(- M + P - P')\cos A.$$

Disturbing force towards the starboard side

$$= (S + S') + I\cos\delta(M + P - P')\sin A.$$

The constant terms will be destroyed by making $H' = -(H + I \sin\delta . N)$, or $\frac{H'}{I\cos\delta} = -\left(\frac{H}{I\cos\delta} + \tan\delta . N\right)$ and $\frac{S'}{I\cos\delta} = -\frac{S}{I\cos\delta}$; which determine (in terms of quantities found from the experiments) the ratios which the directive forces of the magnet or magnets introduced are to bear to the terrestrial directive force. If N have a sensible value, the value of H' here found is strictly correct for one magnetic latitude only. The distance at which a given magnet must be placed will be determined on shore by placing it transverse to the meridian, and determining by trial the distance from a compass at which it makes the tangent of the deviation equal to the ratio required.

In the variable terms, the utmost that we can do is to make $P - P' = 0$, or $P' = P$. The easiest way of determining the distance of the mass of iron which shall produce this effect is by experiment on the angular deviation which it will produce. For the effect depending on P only produces a force towards the east $= I\cos\delta . P . \sin 2A$. the terrestrial directive force is $I\cos\delta$; therefore the deviation (supposed small,) of a compass otherwise undisturbed, is, in terms of radius, $P . \sin 2A$; or, in degrees, $57^\circ\!\cdot\!3 \times P . \sin 2A$. If $A = 45^\circ$, this is $57^\circ\!\cdot\!3 \times P$. But P is known from the experiments. Hence we have this rule. Having a compass on shore, place the mass of iron, which is to be mounted for corrector, at the level of the compass, and in azimuth 45°; find by trial the distance at which it will cause the compass to deviate through $57^\circ\!\cdot\!3 \times P$; that is the distance from the ship's compass at which it must be mounted on the starboard or larboard side.

There still remain uncorrected the following forces:

Towards the ship's head $-I\cos\delta . M . \cos A$.
Towards the starboard side $I\cos\delta . M . \sin A$.

From these we obtain,

Remaining disturbing force towards the north $-I\cos\delta . M$.
Remaining disturbing force towards the east 0.

The direction of the needle, therefore, is not disturbed; these terms express only the diminution of the terrestrial directive force of which we have already spoken.

If it be desired to correct the compass accurately for all magnetic latitudes, the following is the course which must be pursued.

1. From experiments similar to those already described, determine in two different magnetic latitudes the numerical values of

$$\frac{H}{I\cos\delta} + \tan\delta . N, \text{ and } \frac{H}{I'\cos\delta'} + \tan\delta' . N:$$

and from these find H and N.

2. From experiments on a compass on shore, with a mass of iron not in the same level, and in azimuth 90°, determine a position in which it will produce the deviation $57^{\circ}{\cdot}3 \times \tan\delta . N$. This is a position in which it is to be fixed in the ship; on the aft side of the compass if N is positive, or the fore side if N is negative.

3. With the mass of iron at that position, try the deviations which it produces on a compass on shore in azimuth 45° and 135°, and take half the algebraical excess of the former above the latter. This is to be added to the angle $57^{\circ}{\cdot}3 \times P$, to produce the angle which the other mass on the starboard or larboard side is to correct.

4. The magnet introduced as corrector must have an intensity H' equal to $-H$.

It may be proper now to say a few words relative to the application of our theory to the observations of the dipping-needle, as made on the deck of the Rainbow.

The resolved part of the terrestrial force parallel to the ship's keel is $I\cos\delta . \cos A$, and the resolved part vertically downwards is $I\sin\delta$. Combining these with the forces in those directions produced by the permanent and induced magnetism in the ship, we have

$$\text{Cotangent of dip towards ship's head} = \frac{\text{Force towards ship's head}}{\text{Force vertically downwards}}.$$

$$= \frac{I\cos\delta . \cos A + H + I\sin\delta . N + I\cos\delta . (-M + P)\cos A}{I\sin\delta + V - I\sin\delta . Q + I\cos\delta . N . \cos A}$$

$$= \frac{\cot\delta(1 - M + P)\cos A + \cot\delta\left(\frac{H}{I\cos\delta} + \tan\delta . N\right)}{1 + \frac{V}{I\sin\delta} - Q + \cot\delta . N . \cos A}.$$

Let c be the observed cotangent of the dip towards the ship's head; the equation then gives

$$\cot\delta(1 - M + P)\cos A + \cot\delta\left(\frac{H}{I\cos\delta} + \tan\delta . N\right) = c\left(1 + \frac{V}{I\sin\delta} - Q\right)$$
$$+ c . \cot\delta . N\cos A.$$

Substituting in this the values of $(-M + P)$ and $\left(\frac{H}{I\cos\delta} + \tan\delta . N\right)$ found from the observations of intensity, we shall be able to ascertain the values of $1 + \frac{V}{I\sin\delta} - Q$ and N. Where the dipping-needle is incorrect, the best value of δ will be the incorrect dip as shown by the needle.

In like manner we find

$$\text{Cotangent of dip towards starboard side} = \frac{-I\cos\delta . \sin A + S + I\cos\delta . (M + P)\sin A}{I\sin\delta + V - I\sin\delta . Q + I\cos\delta . N . \cos A}$$

$$= \frac{-\cot\delta . (1 - M - P) . \sin A + \cot\delta . \frac{S}{I\cos\delta}}{1 + \frac{V}{I\sin\delta} - Q + \cot\delta . N . \cos A}.$$

Let c' be the observed cotangent of the dip towards the starboard side; the equation then gives

$$-\cot\delta\,(1-M-P).\sin A+\cot\delta.\frac{S}{I\cos\delta}=c'\left(1+\frac{V}{I\sin\delta}-Q\right)+c'.\cot\delta.N.\cos A,$$

which will assist, in the same manner as the last, to give values of $1+\frac{V}{I\sin\delta}-Q$ and N.

With regard to the effect of the ship's heeling, the following investigation appears sufficient. Supposing a magnet to be introduced, parallel to the masts, and with its marked end downwards, its effect downwards and parallel to the masts will be represented by $-V'$. Combining this with the force V of the ship in the same direction, and supposing the angle of heel to starboard to be h, we have for the force to starboard in a horizontal plane

$$\sin h\,(V'-V).$$

Combining these with the forces found in Section III, we have

Force towards ship's head, depending on the heeling,

$$\sin h.I\cos\delta.N.\sin A.$$

Force towards starboard side, depending on the heeling,

$$\sin h\,(V'-V-I\cos\delta.N.\cos A-I\sin\delta.R).$$

As we cannot ensure the neutralization of the term N by the induced magnetism of another mass of iron, (except by experiments in different magnetic latitudes) we must suppose it to exist uncorrected. It is probable that the effect of the forces depending on it will not be great. Their proportion to the directive force will be

$\sin h.N.\sin A$ for the force to the head,

and $-\sin h.N.\cos A$ for the force to the starboard side; or

$$-\sin h.N.\cos 2A$$

for the force to the magnetic east. This proportion does not increase in the high magnetic latitudes.

The proportion of the remaining force $\sin h\,(V'-V-I\sin\delta.R)$ to the directive force, which is expressed by $\sin h\left(\frac{V'-V}{I\cos\delta}-\tan\delta.R\right)$, becomes great in the high magnetic latitudes, and this force therefore ought not to be neglected. From the observations with the dipping-needle, as mentioned above, we may probably obtain the value of V, or rather $V-I\sin\delta.Q$, and may, therefore, determine V' so as partially to neutralize it. Still the term $\tan\delta.R$ remains: and therefore the correction would be imperfect. It would probably be best, therefore, to attempt the correction tentatively (as mentioned before, for wood-built ships). The vessel being placed with her head to the north, and made to heel through the angle h, and the place of a given magnet (arranged as before mentioned) being determined so as to correct any new deviation depending on the heeling, $V' = V + I \cos \delta . N + I \sin \delta . R$. As we cannot easily separate the different terms of this expression, there appears no way of providing for the correction in different magnetic latitudes but by trial in each.

If V should have a sensible magnitude, the principal part of the error of the compass at any place of the ship, when the ship is heeling, will depend on V. If it appears from other of the investigations, that V is small, we may be assured that the deviation depending upon the ship's heeling will not, except in high latitudes, be very great.

Section V.—*Application of the Theory to the Observations made in the Rainbow.*

As the disturbance of the compass was not observed in the experiments for intensity, it has been interpolated graphically between those previously observed. From the times of vibration given in Section II. the following numbers are computed by the methods of Section IV.

No. of station.	Azimuth of ship's head.	Disturbance of compass to East.	$\frac{\text{Intensity to N.}}{\text{I cos } \delta}$.	$\frac{\text{Disturbing force to N.}}{\text{I cos } \delta}$.	$\frac{\text{Disturbing force to E}}{\text{I cos } \delta}$.	$\frac{\text{Dist. force to ship's head}}{\text{I cos } \delta}$.	$\frac{\text{Dist. force to starb. side}}{\text{I cos } \delta}$
I.	0°	− 38° 0′	+ 0·18	− 0·82	− 0·13	− 0·82	− 0·13
	90	− 33 50	+ 1·15	+ 0·15	− 0·77	− 0·77	− 0·15
	180	+ 6 0	+ 1·83	+ 0·83	+ 0·19	− 0·83	− 0·19
	270	+ 45 30	+ 0·77	− 0·23	+ 0·79	− 0·79	− 0·23
II.	0	− 17 0	+ 0·68	− 0·32	− 0·21	− 0·32	− 0·21
	90	− 14 0	+ 1·14	+ 0·14	− 0·28	− 0·28	− 0·14
	180	+ 9 20	+ 1·34	+ 0·34	+ 0·22	− 0·34	− 0·22
	270	+ 24 0	+ 0·73	− 0·27	+ 0·32	− 0·32	− 0·27
III.	0	− 12 30	+ 0·82	− 0·18	− 0·18	− 0·18	− 0·18
	90	− 12 40	+ 1·14	+ 0·14	− 0·26	− 0·26	− 0·14
	180	+ 7 20	+ 1·32	+ 0·32	+ 0·17	− 0·32	− 0·17
	270	+ 23 0	+ 0·72	− 0·28	+ 0·31	− 0·31	− 0·28
IV.	0	− 9 45	+ 0·89	− 0·11	− 0·15	− 0·11	− 0·15
	90	− 7 10	+ 1·15	+ 0·15	− 0·14	− 0·14	− 0·15
	180	+ 8 30	+ 1·18	+ 0·18	+ 0·18	− 0·18	− 0·18
	270	+ 10 15	+ 0·66	− 0·35	+ 0·12	− 0·12	− 0·35

It is impossible to glance at the numbers in the two last columns without noticing the smallness of the variation in each of the disturbing forces, as referred to lines of the ship, while the ship was turned round. From the moment when this Table was completed, it was perfectly clear that the explanation of the principal part of the disturbances of the compass was to be sought in the permanent magnetism of the ship.

Confining ourselves for the present to Station I., we have by the formulæ of the last section,

$$\left(\frac{H}{I \cos \delta} + \tan \delta . N\right) + (-M + P) = -0·82$$

$$\left(\frac{H}{I \cos \delta} + \tan \delta . N\right) \qquad = -0·77$$

$$\left(\frac{H}{I \cos \delta} + \tan \delta . N\right) - (-M + P) = -0·83$$

$$\left(\frac{H}{I\cos\delta} + \tan\delta\,.\,N\right) = -0{\cdot}79$$

$$\frac{S}{I\cos\delta} = -0{\cdot}13$$

$$\frac{S}{I\cos\delta} + (M + P) = -0{\cdot}15$$

$$\frac{S}{I\cos\delta} = -0{\cdot}19$$

$$\frac{S}{I\cos\delta} - (M + P) = -0{\cdot}23$$

The most probable values appear to be

$$\frac{H}{I\cos\delta} + \tan\delta\,.\,N = -0{\cdot}80$$

$$-M + P = 0{\cdot}00$$

$$\frac{S}{I\cos\delta} = -0{\cdot}17$$

$$M + P = 0{\cdot}04$$

whence

$$M = 0{\cdot}02, \qquad P = 0{\cdot}02.$$

The two last quantities are so small that the experiments can hardly be supposed to give their precise values. The value of M indicates that, when the direction is corrected, the intensity will be enfeebled $\frac{1}{50}$ part: that of P shows that, supposing the direction corrected by magnets only, there will be a deviation of the compass changing sign at the alternate quadrants, whose maximum is little more than 1°, and which is such that the needle turns to the right, or observed azimuths appear too small, when the azimuth of the ship's head is between 0 and 90°.

To prove these results completely, the following calculations were made. The two permanent magnetic forces represented by $-0{\cdot}80$ parallel to the ship's keel, and $-0{\cdot}17$ transverse to it, may be compounded into one force $-0{\cdot}82$, making an angle α with the ship's keel, where $\tan\alpha = \frac{17}{80}$ (the horizontal part of terrestrial magnetism being represented by 1·00). A line being taken to represent 1·00, with one end of this line for centre and with radius 0·82, a circle was described; upon this circle the angle α was graphically determined, and the different angles $A + \alpha$ were laid down (A being the azimuth of the ship's head) for every observation from No. 1. to 34. Each of the points on the circle thus determined was joined with the other end of the line, whose length $= 1{\cdot}00$. It is clear that, if the force acting on the north end of the compass be the force compounded of terrestrial magnetism invariable in direction and magnitude, and of ship's magnetism invariable in magnitude, but changing direction with the ship, the resulting actual force will be represented by the joining line last drawn. The angles made by that joining line with the line 1·00 ought, therefore, to be the same as the observed disturbance of compass, or if there is any certain difference, that difference ought to be explainable by the term P mentioned above. The following table contains the values of the angles made by the joining line with the line 1·00 (as taken from the graphical construction), or theoretical disturbance of the compass, compared with the observed disturbance; the sign + denotes that the needle is turned to the left.

No. of observation.	Azimuth of ship's head.	Observed disturbance of compass.	Computed disturbance of compass.	Disturbance yet to be accounted for.	No. of observation.	Azimuth of ship's head.	Observed disturbance of compass.	Computed disturbance of compass.	Disturbance yet to be accounted for.
1	203° 5′	−16° 50′	−15° 50′	−1° 0′	19	1° 35′	+40° 45′	+43° 0′	−2° 15′
2	214 53	−22 45	−21 0	−1 45	20	19 25	+51 20	+55 0	−3 40
3	220 0	−23 20	−23 25	+0 5	21	38 25	+50 50	+52 40	−1 50
4	230 0	−30 5	−27 40	−2 25	22	57 45	+46 30	+47 5	−0 35
5	240 50	−34 5	−32 20	−1 45	23	76 55	+39 15	+39 50	−0 35
6	248 50	−36 40	−35 35	−1 5	24	93 10	+33 5	+33 5	0 0
8	256 4	−40 2	−38 30	−1 32	25	109 0	+26 40	+26 15	+0 25
9	267 44	−44 27	−43 15	−1 12	26	125 50	+19 40	+19 0	+0 40
10	276 12	−47 50	−46 30	−1 20	27	142 10	+11 55	+11 50	+0 5
11	282 57	−49 27	−48 35	−0 52	28	159 10	+ 4 20	+ 4 0	+0 20
12	292 17	−52 25	−51 25	−1 0	29	171 5	− 1 35	− 1 20	−0 15
13	294 0	−53 45	−51 50	−1 55	30	189 45	−11 0	− 9 55	−1 5
14	300 35	−54 50	−53 40	−1 10	31	202 5	−16 45	−15 25	−1 20
15	308 0	−53 50	−54 40	+0 50	32	220 25	−25 10	−23 40	−1 30
16	313 47	−54 2	−54 50	+0 48	33	248 55	−37 50	−35 40	−2 10
17	321 40	−50 10	−53 40	+3 30	34	278 15	−48 25	−47 10	−1 15
18	338 25	−33 0	−35 30	+2 30					

It is quite evident from this table, first, that almost the whole disturbance of the compass is accounted for by the permanent magnetism; secondly, that the residual part follows with sufficient approximation the law of changing signs at the successive quadrants (modified in a small degree by the disturbance in the needle's direction), and with a maximum value not much different from the value 1° already predicted from the observations of intensity. In the quadrants adjacent to the azimuth 0° the maximum appears greater than this quantity, as it ought to appear, in consequence of the great diminution in the needle's directive power.

There remained only, for the complete verification of the theory, to effect an actual correction of the compass. With a two-feet bar magnet placed transversely to the magnetic meridian, experiments were made at Greenwich for ascertaining the distance at which it must be placed below a compass in order to make the needle deviate through 39° 25′ (the angle whose natural tangent = 0·82, the proportion of the ship's permanent magnetic force to the terrestrial directive force at Deptford). The magnet* was then placed in a groove, cut in a board, and making with the edge of the board the angle whose tangent $= \frac{17}{80}$. This mode of mounting was adopted for facility of fixing in the ship, as it was then necessary only to make the edge of the board parallel to the ship's keel, the magnet being at the proper distance below the compass. A roll of iron plate was also prepared, tolerably free from permanent magnetism, and experiments were made to ascertain the distance from a compass at which in azimuth 45° it would cause a deviation something greater than 1°; and arrangements were made for fixing this roll on one side of the ship's compass, in the direction nearly transverse to the keel, and at the same height as the compass card.

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* I know not whether the following observation possesses any novelty or interest. A pair of 2-feet bar magnets had been constructed for me a year or more before this time by Messrs. WATKINS and HILL. On trial, one was found to have not more than half the intensity of the other: it was retouched by the makers: it was again found to be feeble, was again touched; and had been laid up with its fellow and with the keepers on for some months. When trials were made for ascertaining the distance at which the magnets intended for

The magnet being placed in the proper position and at the assigned distance below the compass, and the roll of iron plate being mounted, on making observations in the usual way, it was found (without waiting for the complete calculations) that the deviation was over-corrected. Whether any error had occurred in the measures, or whether the inductive action of the ship increased the power of the magnet (which was placed with its poles in the direction opposite to those of the ship) I do not know. The distance of the magnet was increased, its direction being carefully preserved, and then a series of observations was made and reduced in the usual manner. The results are contained in the following table.

No. of observation.	True azimuth of ship's head.	Disturbance of compass.	No. of observation.	True azimuth of ship's head.	Disturbance of compass.
171	152° 6′	+0° 54′	177	334° 22′	−1° 22′
172	176 56	+0 34	178	357 34	−0 4
173	210 56	−0 41	179	26 34	−0 14
174	239 51	+0 9	180	56 6	+1 54
175	269 53	−0 8	181	87 51	+1 9
176	300 36	−0 6	182	118 36	+0 24

The correction may be considered as perfect as it is possible to make from observations of no greater delicacy than those on which the position of the correctors was determined. I think it is evident that the true maximum error does not exceed half a degree.

Proceeding in the same manner with Station II., we find the following equations.

$$\left(\frac{H}{I\cos\delta} + \tan\delta \, . \, N\right) + (-M + P) = -0{\cdot}32$$

$$\left(\frac{H}{I\cos\delta} + \tan\delta \, . \, N\right) = -0{\cdot}28$$

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correcting the ship's compass should be placed, the keepers were taken off, and the intensity of this magnet was found fully equal to that of its fellow. It was left during one night standing vertically with its marked end upwards: the next morning it had lost two-thirds of its power. I have since examined the powers of several pairs of magnets shortly after taking off their keepers and after the lapse of two or three days, and in every instance one magnet of each pair has lost much of its power.

$$\left(\frac{H}{I\cos\delta} + \tan\delta\,.\,N\right) - (-M + P) = -0{\cdot}34$$

$$\left(\frac{H}{I\cos\delta} + \tan\delta\,.\,N\right) = -0{\cdot}32$$

$$\frac{S}{I\cos\delta} = -0{\cdot}21$$

$$\frac{S}{I\cos\delta} + (M + P) = -0{\cdot}14$$

$$\frac{S}{I\cos\delta} = -0{\cdot}22$$

$$\frac{S}{I\cos\delta} - (M + P) = -0{\cdot}27$$

The most probable values appear to be

$$\frac{H}{I\cos\delta} + \tan\delta\,.\,N = -0{\cdot}32$$

$$-M + P = +0{\cdot}01$$

$$\frac{S}{I\cos\delta} = -0{\cdot}21$$

$$M + P = +0{\cdot}065$$

whence $M = 0{\cdot}03$, $P = 0{\cdot}04$. The last term denotes that the disturbance not depending on permanent magnetism would amount at its maximum to 2°¼, changing signs in successive quadrants, and being negative (in its effects on apparent azimuth) when the azimuth of the ship's head is between 0 and 90°. The large terms show that the permanent magnetism (as compared with the terrestrial force) may be represented by 0·38, inclined to the direction of the keel by an angle α where $\tan\alpha = \frac{21}{32}$. Computing as for Station I., we get the following table of comparisons.

No. of observation.	Azimuth of ship's head.	Observed disturbance of compass.	Computed disturbance of compass.	Disturbance yet to be accounted for.	No. of observation.	Azimuth of ship's head.	Observed disturbance of compass.	Computed disturbance of compass.	Disturbance yet to be accounted for.
35	282° 51′	−21° 36′	−20° 5′	−1° 31′	45	103° 36′	+12° 54′	+11° 50′	+1° 4′
36	300 4	−12 41	−14 50	+2 9	46	124 26	+ 7 26	+ 6 10	+1 16
37	320 49	− 1 19	− 3 55	+2 36	47	143 57	+ 1 26	+ 1 0	+0 26
38	340 7	+ 9 31	+ 8 5	+1 26	48	163 16	− 4 16	− 4 30	+0 14
39	353 48	+15 4	+15 5	−0 1	49	179 6	− 9 6	− 8 50	−0 16
40	11 26	+18 34	+20 30	−1 56	50	203 1	−16 16	−14 45	−1 31
41	31 14	+19 46	+22 35	−2 49	51	222 21	−21 51	−18 55	−2 56
42	49 47	+19 51	+21 50	−1 59	52	244 56	−26 26	−22 5	−4 21
43	67 3	+18 12	+19 35	−1 23	53	267 46	−24 16	−22 30	−1 46
44	85 1	+14 59	+16 15	−1 16					

The residual term here follows with great accuracy the law of the deviation depending on P, and its magnitude is, as nearly as the nature of the observation allows, the same as that predicted.

For Station III.

$$\left(\frac{H}{I\cos\delta} + \tan\delta\,.\,N\right) + (-M + P) = -0{\cdot}18$$

$$\left(\frac{H}{I\cos\delta} + \tan\delta\,.\,N\right) = -0{\cdot}26$$

$$\left(\frac{H}{I\cos\delta} + \tan\delta\,.\,N\right) - (-M + P) = -0{\cdot}32$$

$$\left(\frac{H}{I\cos\delta} + \tan\delta\,.\,N\right) = -0{\cdot}31$$

$$\frac{S}{I\cos\delta} = -0{\cdot}18$$

$$\frac{S}{I\cos\delta} + (M + P) = -0{\cdot}14$$

$$\frac{S}{I\cos\delta} = -0{\cdot}17$$

$$\frac{S}{I\cos\delta} - (M + P) = -0{\cdot}28$$

The most probable values are

$$\frac{H}{I\cos\delta} + \tan\delta\,.\,N = -0{\cdot}27$$

$$-M + P = +0{\cdot}07$$

$$\frac{S}{I\cos\delta} = -0{\cdot}19$$

whence

$$M + P = +0{\cdot}07$$

$$M = 0{\cdot}00, \qquad P = 0{\cdot}07.$$

The term of the disturbance of the compass which changes sign in the alternate quadrant ought therefore to have a maximum value of 4°; its sign, in each quadrant, ought to be the same as at Station I. and II. The permanent magnetism of the ship is represented by 0·33, inclined to the ship's keel at an angle whose natural tangent is $\frac{19}{27}$. On computing the effect of this independent magnetism, in the same manner as for Station I., the following Table is formed.

No. of observation.	Azimuth of ship's head.	Observed disturbance of compass.	Computed disturbance of compass.	Disturbance yet to be accounted for.	No. of observation.	Azimuth of ship's head.	Observed disturbance of compass.	Computed disturbance of compass.	Disturbance yet to be accounted for.
54	284° 11′	−19° 41′	−16° 10′	−3° 31′	64	93° 11′	+11° 49′	+12° 30′	−0° 41′
55	300 24	−13 24	−11 30	−1 54	65	120 36	+ 7 54	+ 6 10	+1 44
56	311 18	− 4 18	− 6 45	+2 27	66	139 46	+ 3 44	+ 1 35	+2 9
57	324 26	+ 2 49	− 0 35	+3 24	67	163 9	− 3 9	− 4 30	+1 21
58	340 51	+ 7 39	+ 7 30	+0 9	68	187 9	−10 39	− 9 55	−0 44
59	2 34	+11 56	+15 15	−3 19	69	216 6	−18 21	−15 50	−2 31
60	22 16	+14 29	+18 40	−4 11	70	235 11	−21 41	−18 20	−3 21
61	38 11	+16 19	+19 25	−3 6	71	259 59	−24 29	−19 20	−5 9
62	60 34	+15 26	+17 55	−2 29	72	275 41	−21 41	−17 40	−4 1
63	81 36	+13 24	+14 40	−1 16					

Remarking that the residual errors with the sign − exceed in magnitude and number those with the sign +, I conjecture that the index error has been erroneously determined, to the extent perhaps of 40′ or 50′. With this supposition, the agreement of the observed residual term with the theoretical residual term is very close.

For Station IV. we have as follows:

$$\left(\frac{\mathrm{H}}{\mathrm{I}\cos\delta}+\tan\delta\,.\,\mathrm{N}\right)+(-\mathrm{M}+\mathrm{P})=-0{\cdot}11$$

$$\left(\frac{\mathrm{H}}{\mathrm{I}\cos\delta}+\tan\delta\,.\,\mathrm{N}\right)=-0{\cdot}14$$

$$\left(\frac{\mathrm{H}}{\mathrm{I}\cos\delta}+\tan\delta\,.\,\mathrm{N}\right)-(-\mathrm{M}+\mathrm{P})=-0{\cdot}18$$

$$\left(\frac{\mathrm{H}}{\mathrm{I}\cos\delta}+\tan\delta\,.\,\mathrm{N}\right)=-0{\cdot}12$$

$$\frac{\mathrm{S}}{\mathrm{I}\cos\delta}=-0{\cdot}15$$

$$\frac{\mathrm{S}}{\mathrm{I}\cos\delta}+(\mathrm{M}+\mathrm{P})=-0{\cdot}15$$

$$\frac{\mathrm{S}}{\mathrm{I}\cos\delta}=-0{\cdot}18$$

$$\frac{\mathrm{S}}{\mathrm{I}\cos\delta}-(\mathrm{M}+\mathrm{P})=-0{\cdot}35$$

The most probable values appear to be

$$\frac{\mathrm{H}}{\mathrm{I}\cos\delta}+\tan\delta\,.\,\mathrm{N}=-0{\cdot}14$$

$$-\mathrm{M}+\mathrm{P}=+0{\cdot}035$$

$$\frac{\mathrm{S}}{\mathrm{I}\cos\delta}=-0{\cdot}21$$

$$\mathrm{M}+\mathrm{P}=+0{\cdot}10$$

whence $\mathrm{M}=0{\cdot}03,\qquad \mathrm{P}=0{\cdot}07.$

The coefficient therefore of the term which changes signs at alternate quadrants ought to be 4° nearly; the permanent magnetism ought to be represented by 0·25 inclined at an angle whose tangent $=\frac{21}{14}$. The following Table will show the comparison of the term computed (as before) from the permanent magnetism with the disturbance observed.

No. of observation.	Azimuth of ship's head.	Observed disturbance of compass.	Computed disturbance of compass.	Disturbance yet to be accounted for.	No. of observation.	Azimuth of ship's head.	Observed disturbance of compass.	Computed disturbance of compass.	Disturbance yet to be accounted for.
73	96° 21′	− 9° 21′	− 8° 20′	−1° 1′	83	310° 6′	+ 3° 54′	− 1° 30′	+5° 24′
74	111 6	− 2 6	− 4 5	+1 59	84	326 46	− 0 46	− 4 50	+4 4
76	150 36	+ 8 54	+ 8 35	+0 19	85	350 16	− 6 16	− 9 10	+2 54
77	167 6	+ 8 54	+12 15	−3 21	86	18 1	−12 31	−13 0	+0 29
78	183 41	+ 9 49	+14 20	−4 31	87	39 36	−15 21	−14 45	−0 36
79	196 46	+10 14	+14 40	−4 26	88	65 9	−15 39	−14 0	−1 39
80	229 54	+ 7 36	+12 50	−5 14	89	84 34	−12 4	−11 0	−1 4
81	246 59	+ 8 1	+10 20	−2 19					
82	275 6	+ 7 9	+ 5 50	+1 19					

The agreement of the residual term here with the theoretical residual term is not so perfect as at the other stations, but is nevertheless very close. A trifling alteration in the independent magnetism would make the agreement perfect. I may observe that, *à priori*, the existence of a disagreement at this station is probable. The observations of intensity and of disturbance of the compass were not made at the same time; and it is extremely probable that the box of chain-cable (mounted on wheels upon the deck, at no great distance from Station IV.) had been moved in the intermediate time.

Attempts were made to correct the compass at Stations II., III., and IV., in the same manner as at Station I. They did not, however, succeed so perfectly. I attribute this failure to the following cause. As the observations at Station I. had seemed to show that a small tentative adjustment might be necessary, a tentative adjustment was tried, depending of course upon the indications of the compass, at each of the other stations before making a round of observations. The compass it was found had become exceedingly sluggish (probably from the blunting of the suspending point by very frequent movements), two readings for the same bearing being sometimes obtained differing 8° or more. By one of these errors an adjustment might be affected, and its consequence would be found in every observation made while the magnets were in the position so adjusted. As a general rule it is to be recommended that no tentative adjustment be made, except the whole apparatus is in the highest possible order. When the vessel was prepared for sea, three compasses (at Stations I. II. and III.) were fitted with magnets and masses of soft iron placed according to the directions in the preceding section, and arranged, as to intensity and position of the magnets, and as to maximum effect of the soft iron, in accordance with the numbers just found; and the direction of these three compasses has appeared to be, as far as general observation could discover, quite correct. The compass at Station II. occasionally differed from the others two or three degrees; but this error might be occasioned by a blow which the magnet received, or might be produced by the effect of the powerful 2-feet magnet under Station I. The magnets under Stations II. and III. were 14-inch magnets, producing no sensible effect at any station but those to which they belonged.

On the whole, I conclude that the explanation of the deviations of the compass, by the combined powers of independent magnetism of the ship and induced magnetism produced by terrestrial action, is perfect; and that there is no reason to doubt that by the introduction of antagonist magnets and masses of soft iron the correction may be made perfect.

I shall now proceed with the discussion of the observations made with the dipping-needle.

The equation depending on the dip towards the ship's head given in the last section, being applied to Station I., and the substitution of 0·00 for $-M+P$, 0·80 for $\frac{H}{I\cos\delta}+\tan\delta\,.\,N$, and 72° 40′ for δ, being made, the equation assumes the following form:

$$0{\cdot}312\,.\cos A - 0{\cdot}250 = c\left(1+\frac{V}{I\sin\delta}-Q\right)+0{\cdot}312\,.\,c\,.\,N\,.\cos A.$$

Substituting in each of the observations from No. 35 to 53, for Station I., we have the following equations.

No. of observation.	Equation.
35	$-0{\cdot}181 = -0{\cdot}197\left(1+\frac{V}{I\sin\delta}-Q\right) - {\cdot}014\,N.$
37	$-0{\cdot}008 = -0{\cdot}047 \ldots\ldots\ldots\ldots - {\cdot}011 \ldots$
38	$+0{\cdot}043 = +0{\cdot}010 \ldots\ldots\ldots\ldots + {\cdot}003 \ldots$
39	$+0{\cdot}060 = +0{\cdot}032 \ldots\ldots\ldots\ldots + {\cdot}010 \ldots$
40	$+0{\cdot}056 = +0{\cdot}033 \ldots\ldots\ldots\ldots + {\cdot}010 \ldots$
41	$+0{\cdot}017 = -0{\cdot}015 \ldots\ldots\ldots\ldots - {\cdot}004 \ldots$
42	$-0{\cdot}048 = -0{\cdot}067 \ldots\ldots\ldots\ldots - {\cdot}013 \ldots$
43	$-0{\cdot}128 = -0{\cdot}132 \ldots\ldots\ldots\ldots - {\cdot}016 \ldots$
44	$-0{\cdot}223 = -0{\cdot}206 \ldots\ldots\ldots\ldots - {\cdot}005 \ldots$
45	$-0{\cdot}323 = -0{\cdot}320 \ldots\ldots\ldots\ldots + {\cdot}023 \ldots$
46	$-0{\cdot}427 = -0{\cdot}404 \ldots\ldots\ldots\ldots + {\cdot}071 \ldots$
47	$-0{\cdot}502 = -0{\cdot}468 \ldots\ldots\ldots\ldots + {\cdot}118 \ldots$
48	$-0{\cdot}548 = -0{\cdot}535 \ldots\ldots\ldots\ldots + {\cdot}160 \ldots$
49	$-0{\cdot}562 = -0{\cdot}552 \ldots\ldots\ldots\ldots + {\cdot}172 \ldots$
50	$-0{\cdot}537 = -0{\cdot}535 \ldots\ldots\ldots\ldots + {\cdot}153 \ldots$
51	$-0{\cdot}480 = -0{\cdot}473 \ldots\ldots\ldots\ldots + {\cdot}109 \ldots$
52	$-0{\cdot}382 = -0{\cdot}392 \ldots\ldots\ldots\ldots + {\cdot}052 \ldots$
53	$-0{\cdot}262 = -0{\cdot}285 \ldots\ldots\ldots\ldots + {\cdot}003 \ldots$

The sum of the equations from 35 to 44, and from 45 to 53, give respectively,

$$-0{\cdot}412 = -0{\cdot}589\left(1+\frac{V}{I\sin\delta}-Q\right) - 0{\cdot}030\,.\,N.$$

$$-4{\cdot}023 = -3{\cdot}964\left(1+\frac{V}{I\sin\delta}-Q\right) + 0{\cdot}861\,.\,N.$$

When the rudeness of the observations is considered, it will be evident that the first of these equations is wholly unfit (from the smallness of its coefficients) to be combined with the latter for the determination of two unknown quantities. If we add them together we find

$$-4{\cdot}435 = -4{\cdot}553\left(1+\frac{V}{I\sin\delta}-Q\right) + 0{\cdot}831\,.\,N$$

and if we assume N to be small (which assumption appears, from the nature of the integral on which it depends, to be well founded), we must infer that the vertical part of the ship's permanent magnetism is very small.

The equation for the transversal dip, substituting 0·04 for M + P and − 0·17 for $\frac{S}{I \cos \delta}$, becomes

$$- 0{\cdot}300 \,.\, \sin A - 0{\cdot}053 = c' \left(1 + \frac{V}{I \sin \delta} - Q\right) + 0{\cdot}312 \,.\, c' \,.\, N \,.\, \cos A.$$

Substituting in each of the observations from 54 to 72, we get the following equations.

No. of observation.	Equation.
54	$+ {\cdot}238 = + {\cdot}203 \left(1 + \frac{V}{I \sin \delta} - Q\right) + {\cdot}015\ N.$
55	$+ {\cdot}217 = + {\cdot}190$ .... $+ {\cdot}024$ ..
56	$+ {\cdot}172 = + {\cdot}114$ .... $+ {\cdot}024$ ..
57	$+ {\cdot}121 = + {\cdot}079$ .... $+ {\cdot}020$ ..
58	$+ {\cdot}045 = + {\cdot}016$ .... $+ {\cdot}005$ ..
59	$- {\cdot}068 = - {\cdot}070$ .... $- {\cdot}022$ ..
60	$- {\cdot}167 = - {\cdot}176$ .... $- {\cdot}051$ ..
61	$- {\cdot}238 = - {\cdot}245$ .... $- {\cdot}060$ ..
62	$- {\cdot}314 = - {\cdot}312$ .... $- {\cdot}048$ ..
63	$- {\cdot}350 = - {\cdot}381$ .... $- {\cdot}017$ ..
64	$- {\cdot}352 = - {\cdot}344$ .... $+ {\cdot}006$ ..
65	$- {\cdot}311 = - {\cdot}315$ .... $+ {\cdot}050$ ..
66	$- {\cdot}247 = - {\cdot}249$ .... $+ {\cdot}059$ ..
67	$- {\cdot}140 = - {\cdot}196$ .... $+ {\cdot}058$ ..
68	$- {\cdot}016 = - {\cdot}041$ .... $+ {\cdot}013$ ..
69	$+ {\cdot}126 = + {\cdot}087$ .... $- {\cdot}022$ ..
70	$+ {\cdot}193 = + {\cdot}148$ .... $- {\cdot}026$ ..
71	$+ {\cdot}243 = + {\cdot}196$ .... $- {\cdot}011$ ..
72	$+ {\cdot}246 = + {\cdot}203$ .... $- {\cdot}006$ ..

The sum of the equations from 54 to 58 gives

$$(1) + {\cdot}793 = + {\cdot}602 \left(1 + \frac{V}{I \sin \delta} - Q\right) + {\cdot}088 \,.\, N.$$

The sum of the equations from 59 to 63 gives

$$(2) - 1{\cdot}137 = - 1{\cdot}184 \left(1 + \frac{V}{I \sin \delta} - Q\right) - {\cdot}198 \,.\, N.$$

The sum of the equations from 64 to 68 gives

$$(3) - 1{\cdot}066 = - 1{\cdot}145 \left(1 + \frac{V}{I \sin \delta} - Q\right) + {\cdot}186\ N.$$

The sum of the equations from 69 to 72 gives

$$(4) + {\cdot}808 = + {\cdot}634 \left(1 + \frac{V}{I \sin \delta} - Q\right) - {\cdot}065 \,.\, N.$$

(1) − (2) − (3) + (4) give

$$+ 3{\cdot}804 = + 3{\cdot}565 \left(1 + \frac{V}{I \sin \delta} - Q\right) + {\cdot}035\ N.$$

(1) − (2) + (3) − (4) give

$$+\,0{\cdot}056 = +\,{\cdot}007\left(1 + \frac{\mathrm{V}}{\mathrm{I}\sin\delta} - \mathrm{Q}\right) + {\cdot}537\,\mathrm{N}.$$

From the two last equations, $1 + \frac{\mathrm{V}}{\mathrm{I}\sin\delta} - \mathrm{Q} = 1{\cdot}06$, $\mathrm{N} = +\,{\cdot}09$; which values, with very trifling alterations, would satisfy the equation derived from the longitudinal dips. The excess of the numbers on the first side with the sign + and the defect of those with the sign − might be caused by an erroneous determination of $\frac{\mathrm{S}}{\mathrm{I}\cos\delta}$, but it is far more probable that it has originated here in an error of adjustment of the dipping-needle, made while the ship was heeling: it produces no sensible effect on the solutions of the equations.

The equation for dip towards the ship's head at Station III. becomes, on substituting the numbers proper for that station,

$$0{\cdot}407\cos\mathrm{A} - 0{\cdot}103 = c\left(1 + \frac{\mathrm{V}}{\mathrm{I}\sin\delta} - \mathrm{Q}\right) + c\,.\,0{\cdot}381\cos\mathrm{A}\,.\,\mathrm{N}.$$

Forming the separate equations, combining those from No. 35 to 43 in one group, and those from 44 to 53 in another group, we obtain

$$+\,1{\cdot}629 = +\,1{\cdot}855\left(1 + \frac{\mathrm{V}}{\mathrm{I}\sin\delta} - \mathrm{Q}\right) + 0{\cdot}591\,.\,\mathrm{N}.$$

$$+\,3{\cdot}308 = -\,3{\cdot}260\left(1 + \frac{\mathrm{V}}{\mathrm{I}\sin\delta} - \mathrm{Q}\right) + 0{\cdot}924\,.\,\mathrm{N}.$$

From these, $1 + \frac{\mathrm{V}}{\mathrm{I}\sin\delta} - \mathrm{Q} = 0{\cdot}95$, $\mathrm{N} = -\,0{\cdot}22$.

The equation for dip towards the starboard side at the same station is

$$-\,0{\cdot}354\sin\mathrm{A} - {\cdot}072 = c'\left(1 + \frac{\mathrm{V}}{\mathrm{I}\sin\delta} - \mathrm{Q}\right) + c'\,.\,0{\cdot}381\cos\mathrm{A}\,.\,\mathrm{N}.$$

Grouping the equations from 54 to 57, 60 to 63, 64 to 68, 69 to 71, and 72, and then combining these groups so as to make all the multipliers of the unknown quantities additive, one in each equation, we get the following equations

$$+\,4{\cdot}357 = +\,4{\cdot}416\left(1 + \frac{\mathrm{V}}{\mathrm{I}\sin\delta} - \mathrm{Q}\right) + 0{\cdot}081\,.\,\mathrm{N}.$$

$$+\,0{\cdot}485 = +\,0{\cdot}524\left(1 + \frac{\mathrm{V}}{\mathrm{I}\sin\delta} - \mathrm{Q}\right) + 0{\cdot}777\,.\,\mathrm{N}.$$

From these, $1 + \frac{\mathrm{V}}{\mathrm{I}\sin\delta} - \mathrm{Q} = 0{\cdot}99$, $\mathrm{N} = -\,0{\cdot}04$.

The agreement of these results with those obtained for the dip towards the head is as close as could be expected. The equations for determining N cannot be made very favourable, and great precision in its value is not to be hoped for. The agreement of the two values of $1 + \frac{\mathrm{V}}{\mathrm{I}\sin\delta} - \mathrm{Q}$ is a proof of the accuracy of the determinations of horizontal intensity, and of the general correctness of the theory which connects the different observations. For upon the values of P, deduced from the observations of intensity, the first term in every one of these equations depends (the difference in the numerical values of the coefficients 0·407 and 0·354 depends entirely upon it): a very small alteration of P would make the results for $1 + \frac{V}{I \sin \delta} - Q$ precisely equal: the omission of P would have caused very great discordance in the values of $1 + \frac{V}{I \sin \delta} - Q$.

I have not been equally successful with the observations at Station IV. The dipping-needle used there was, as I have mentioned, in the proximity of large moveable masses of iron, the windlass, the anchors, and the box of chain-cable. Whatever the cause may be, the observations are in themselves discordant. Upon trying whether the cotangent of the dip could be approximately represented by $a \cos A + b$ for dips towards the head, and by $a' \sin A + b'$ for dips towards the starboard side, it was found that the discordances were between three and four times as great at Station IV. as at Station III. I have not thought it worth while to occupy space with the equations, &c. at this station; considering that the results already obtained are sufficient for my present purpose.

The quantity Q it will be remarked (from its expression in Section III.) is of the same order as M, and therefore probably small. Hence the assertion that $1 + \frac{V}{I \sin \delta} - Q$ differs little from 1, enables us at once to assert that V, the permanent vertical magnetic force of the ship, is small.

Section VI.—*Observations not essential to the Theory.*

The following account of the observed disturbance of the compass on the upper floor of the stage will probably be sufficient.

At Station I. the disturbance vanished when the azimuth of the ship's head was about 150° and 320°. The greatest + disturbance (needle deviating to the left) took place in azimuth 78° nearly, and amounted to + 5°; the greatest − disturbance occurred in azimuth 260°, and amounted to − 10°.

At Station II. the disturbance vanished in azimuths 156° and 330° nearly. The greatest + disturbance was in azimuth 50° or 55°, and exceeded 10°; the greatest − disturbance was in azimuth 267°, and amounted to 11½°.

At Station III. the disturbance vanished in azimuth 150° and 330° nearly. The greatest + disturbance was in azimuth 76°, and amounted to 7¼°; the greatest − disturbance was in azimuth 230°, and amounted to 9½°.

At Station IV. the disturbance vanished in azimuths 155° and 320° nearly. The greatest + disturbance was in azimuth 60° nearly, and amounted to 8°; the greatest − disturbance was in azimuth 230° nearly, and amounted to about 8°.

These disturbances are evidently referable, for the greatest part of their amount, to permanent magnetism; though the effect of induced magnetism is sensible, especially in the observations at Station I.

The observations made with the dipping-needles and large horizontal magnets on shore, were conducted under circumstances of weather so unfavourable (amidst torrents of rain and very heavy gusts of wind), that it has not appeared worth while to transcribe them. The following result, however, may be stated as deducible from them with perfect certainty. On one or two occasions there appeared to be reason to think that the ship's head, when near, attracted the upper end of the dipping-needle, or the unmarked end of the horizontal magnet, but the effect hardly exceeded the errors of observation. But in every instance when the ship's stern was brought near to the dipping-needles or magnets, it powerfully attracted the lower or marked end. The most distinct estimate of its power may be obtained from the following observation. On August 2, the ship's head being south nearly, and a dipping-needle vibrating in the meridian being placed at the distance 25 feet from the stern, the dip of the needle was increased 7° 30′. I may remark that the dipping-needle is an infinitely less delicate instrument (where horizontal forces only are concerned) than the large horizontal magnet, as used by Gauss, with reflector and graduated scale, suspended by one bundle of silk fibres if parallel to the magnetic meridian, or by two if transverse to it. The dipping-needle, however, is mounted with much greater speed and less trouble than the magnet, and is more easily protected from the violence of the weather.

Section VII.—*Observations of the Disturbance of the Compass in the Iron-built Sailing-ship Ironsides.*

On the 26th and 27th of October 1838, I examined the binnacle compass of the iron sailing-ship Ironsides in the Brunswick Dock, Liverpool. The observations were made by placing one azimuth compass in the position of the binnacle, and another on shore, and with each of these compasses observing a small signal carried by the other. In this manner the reductions for locality, &c. were completely avoided, but every result was liable to the errors of two compasses. The experience of the investigations in the Rainbow having shown the importance of observations of horizontal intensity, I applied the intensity apparatus in every position of the ship in which the compass was observed. The time occupied by forty vibrations of the needle on shore was $195^s\cdot0$. The values for disturbing forces in the following Table are computed by the methods of Section IV.

Reference to observation.	Azimuth of ship's head.	Disturbance of compass.	Time of forty vibrations.	$\frac{\text{Intensity to N.}}{\text{I}\cos\delta}$	$\frac{\text{Dist. force to N.}}{\text{I}\cos\delta}$	$\frac{\text{Dist. force to E.}}{\text{I}\cos\delta}$	$\frac{\text{Dist. force to head}}{\text{I}\cos\delta}$	$\frac{\text{Dist. force to starboard}}{\text{I}\cos\delta}$
a	9° 50′	−28° 20′	262·8^{s}	0·485	−0·515	+0·261	−0·462	+0·345
b	23 0	−22 30	279·4	0·450	−0·550	+0·186	−0·433	+0·387
c	32 35	−11 20	299·2	0·416	−0·584	+0·083	−0·447	+0·385
d	47 40	+ 6 40	303·3	0·411	−0·589	−0·048	−0·432	+0·403
e	58 55	+16 50	289·4	0·435	−0·565	−0·131	−0·405	+0·416
f	78 40	+28 50	256·3	0·507	−0·493	−0·279	−0·371	+0·428
g	89 0	+35 0	228·4	0·597	−0·403	−0·418	−0·425	+0·396
h	112 0	+27 10	204·4	0·810	−0·190	−0·416	−0·314	+0·332
i	151 0	+25 30	179·8	1·062	+0·062	−0·506	−0·299	+0·413
k	171 50	+16 0	171·4	1·244	+0·244	−0·357	−0·293	+0·318
l	184 55	+10 55	166·1	1·353	+0·353	−0·261	−0·330	+0·290
m	211 30	− 0 40	163·0	1·431	+0·431	+0·017	−0·376	+0·211
n	233 30	− 6 20	163·2	1·419	+0·419	+0·157	−0·376	+0·243
o	250 35	−11 50	166·2	1·347	+0·347	+0·282	−0·381	+0·233
p	279 35	−20 20	174·4	1·172	+0·172	+0·434	−0·399	+0·242
q	304 20	−25 20	188·7	0·965	−0·035	+0·457	−0·397	+0·229
r	333 10	−30 30	209·7	0·745	−0·255	+0·439	−0·426	+0·277
s	350 50	−29 20	235·6	0·597	−0·403	+0·336	−0·452	+0·267

Expressing each of the values of $\frac{\text{Disturbing force to head}}{\text{I}\cos\delta}$ by the formula

$$\left(\frac{\text{H}}{\text{I}\cos\delta} + \tan\delta\,.\,\text{N}\right) + (\text{P}-\text{M})\cos\text{A},$$

we get the following series of equations.

Reference to observation.	Equation.
a	$-0·462 = \left(\frac{\text{H}}{\text{I}\cos\delta} + \tan\delta\,.\,\text{N}\right) + 0·985\,(\text{P}-\text{M})$
b	− 0·433 = ... + 0·921........
c	− 0·447 = ... + 0·843........
d	− 0·432 = ... + 0·673........
e	− 0·405 = ... + 0·516........
f	− 0·371 = ... + 0·197........
g	− 0·425 = ... + 0·017........
h	− 0·314 = ... − 0·375........
i	− 0·299 = ... − 0·875........
k	− 0·293 = ... − 0·990........
l	− 0·330 = ... − 0·996........
m	− 0·376 = ... − 0·853........
n	− 0·376 = ... − 0·595........
o	− 0·381 = ... − 0·332........
p	− 0·399 = ... + 0·166........
q	− 0·397 = ... + 0·564........
r	− 0·426 = ... + 0·892........
s	− 0·452 = ... + 0·987........

The sum of all the equations gives

$$-7·018 = 18\left(\frac{\text{H}}{\text{I}\cos\delta} + \tan\delta\,.\,\text{N}\right) + 1·745\,.\,(\text{P}-\text{M}).$$

The sum of all, changing the signs of those from *g* to *p*, gives

$$-0·632 = +11·411\,.\,(\text{P}-\text{M}).$$

From these,

$$\text{P}-\text{M} = -0·055,\quad \frac{\text{H}}{\text{I}\cos\delta} + \tan\delta\,.\,\text{N} = -0·386.$$

Again, expressing each of the values of $\frac{\text{disturbing force to starboard}}{\mathrm{I}\cos\delta}$ by the formula

$$\frac{\mathrm{S}}{\mathrm{I}\cos\delta} + (\mathrm{P} + \mathrm{M})\sin\mathrm{A},$$

we have the following equations.

Reference to observation.	Equation.
a	$+ 0{\cdot}345 = \frac{\mathrm{S}}{\mathrm{I}\cos\delta} + 0{\cdot}171\,.\,(\mathrm{P} + \mathrm{M})$
b	$+ 0{\cdot}387 = \ldots\ldots + 0{\cdot}391 \ldots\ldots\ldots\ldots$
c	$+ 0{\cdot}385 = \ldots\ldots + 0{\cdot}539 \ldots\ldots\ldots\ldots$
d	$+ 0{\cdot}403 = \ldots\ldots + 0{\cdot}739 \ldots\ldots\ldots\ldots$
e	$+ 0{\cdot}416 = \ldots\ldots + 0{\cdot}856 \ldots\ldots\ldots\ldots$
f	$+ 0{\cdot}428 = \ldots\ldots + 0{\cdot}981 \ldots\ldots\ldots\ldots$
g	$+ 0{\cdot}396 = \ldots\ldots + 1{\cdot}000 \ldots\ldots\ldots\ldots$
h	$+ 0{\cdot}332 = \ldots\ldots + 0{\cdot}927 \ldots\ldots\ldots\ldots$
i	$+ 0{\cdot}413 = \ldots\ldots + 0{\cdot}485 \ldots\ldots\ldots\ldots$
k	$+ 0{\cdot}318 = \ldots\ldots + 0{\cdot}142 \ldots\ldots\ldots\ldots$
l	$+ 0{\cdot}290 = \ldots\ldots - 0{\cdot}086 \ldots\ldots\ldots\ldots$
m	$+ 0{\cdot}211 = \ldots\ldots - 0{\cdot}523 \ldots\ldots\ldots\ldots$
n	$+ 0{\cdot}243 = \ldots\ldots - 0{\cdot}804 \ldots\ldots\ldots\ldots$
o	$+ 0{\cdot}233 = \ldots\ldots - 0{\cdot}943 \ldots\ldots\ldots\ldots$
p	$+ 0{\cdot}242 = \ldots\ldots - 0{\cdot}986 \ldots\ldots\ldots\ldots$
q	$+ 0{\cdot}229 = \ldots\ldots - 0{\cdot}826 \ldots\ldots\ldots\ldots$
r	$+ 0{\cdot}277 = \ldots\ldots - 0{\cdot}451 \ldots\ldots\ldots\ldots$
s	$+ 0{\cdot}267 = \ldots\ldots - 0{\cdot}159 \ldots\ldots\ldots\ldots$

The sum of all the equations gives

$$+ 5{\cdot}815 = 18 \cdot \frac{\mathrm{S}}{\mathrm{I}\cos\delta} + 1{\cdot}453\,.\,(\mathrm{P} + \mathrm{M}).$$

The sum of all, changing the signs from *k* to *s*, gives

$$+ 1{\cdot}195 = + 10{\cdot}725\,(\mathrm{P} + \mathrm{M}).$$

From these,

$$\mathrm{P} + \mathrm{M} = + 0{\cdot}111, \quad \frac{\mathrm{S}}{\mathrm{I}\cos\delta} = + 0{\cdot}314.$$

and

$$\mathrm{M} = 0{\cdot}083, \quad \mathrm{P} = 0{\cdot}028.$$

The permanent magnetic force may therefore be represented by the force

$$\sqrt{\{\overline{0{\cdot}386}^2 + \overline{0{\cdot}314}^2\}} = 0{\cdot}50,$$

inclined to the ship's keel at an angle whose tangent $= \frac{-314}{386}$. In compounding this, for the different positions of the ship, with the terrestrial force, it is to be observed that the diminution 0·08 is too serious to be neglected (as we have hitherto done), and that 0·92 must therefore be considered as measuring the terrestrial force. With these numbers the effect of the permanent magnetism is computed graphically as before; and the result, and its comparison with the observed disturbance, are contained in the following Table.

Reference to observation.	Azimuth of ship's head.	Observed disturbance of compass.	Computed disturbance of compass.	Disturbance yet to be accounted for.	Reference to observation.	Azimuth of ship's head.	Observed disturbance of compass.	Computed disturbance of compass.	Disturbance yet to be accounted for.
a	9° 50′	−28° 20′	−27° 20′	−1° 0′	*k*	171° 50′	+16° 0′	+16° 10′	−0° 10′
b	23 0	−22 30	−18 0	−4 30	*l*	184 55	+10 55	+11 45	−0 50
c	32 35	−11 20	− 7 30	−3 50	*m*	211 30	− 0 40	+ 2 30	−3 10
d	47 40	+ 6 40	+10 0	−3 20	*n*	233 30	− 6 20	− 5 5	−1 15
e	58 55	+16 50	+20 45	−3 55	*o*	250 35	−11 50	−11 0	−0 50
f	78 40	+28 50	+30 45	−1 55	*p*	279 35	−20 20	−20 30	+0 10
g	89 0	+35 0	+32 25	+2 35	*q*	304 20	−25 20	−27 20	+2 0
h	112 0	+27 10	+31 30	−4 20	*r*	333 10	−30 30	−32 30	+2 0
i	151 0	+25 30	+22 30	+3 0	*s*	350 50	−29 20	−30 0	+0 40

From the value of P above, the residual term ought to have for maximum value 1°·6, supposing the permanent magnetism not to act at the same time; in simultaneous action the maximum would be nearly doubled in the first quadrant (from the weakening of the terrestrial force by the permanent magnetism), and would be diminished by one-third in the third quadrant; the signs would be − + − + in the four quadrants. The agreement of the signs and magnitudes of the residual terms with those defined by this law is extremely close; the value for observation *h* alone presenting a sensible disagreement. The theory, therefore, may be considered as perfectly in accordance with the facts observed in the deviations and intensities at the position of this compass.

The maximum of the term depending on P being (when the permanent magnetism is counteracted) 1°·6, it was thought unnecessary to encumber the binnacle with a mass of iron for the correction of that small quantity. The only correction applied was a magnet, placed below the compass, in a position making the angle whose tangent is $\frac{-314}{386}$ with the ship's keel, and at a distance at which its effect (as found by trial on shore) was 0·50 of the earth's directive force, or produced a deviation of 27° nearly when transverse to the meridian. The deviation of the compass was then found to be as follows.

Azimuth of ship's head.	Disturbance of compass.
3° 15′	+1° 30′
45 45	−1 15
94 45	−1 45
182 45	−0 45
224 0	−2 30
276 20	+0 40
323 0	−2 30

The whole of these apparent disturbances are within the limits of errors of observation; it is difficult to say whether they contain any distinct trace of the inductive term.

Another compass in the same ship (a tell-tale, or compass suspended to a beam in the cabin) was observed in regard to deviation only. The observations were made by an incompetent person, and are not worth transcribing. The maximum deviation was greater than that at the binnacle. But this singular circumstance presented itself: that the deviation vanished when the azimuth of the ship's head was 140° and 320° nearly, the maximum + error occurring near azimuth 200°, and the maximum − error near azimuth 80°. At the binnacle compass, the deviation vanished in azimuths 40° and 220° nearly, and its maximum + and − errors occurred in azimuths 90° and 340° nearly. Therefore, to make the direction of the ship's independent magnetism at the tell-tale parallel to the magnetic meridian, it was necessary to turn the ship 100 degrees further than was necessary to effect the same for the binnacle compass. Or, supposing the head of the ship towards the top of the page, the direction of the magnetic force (as acting on the marked end of the needle) is ↘ at the binnacle, and ↗ at the tell-tale. These stations are not, if I remember right, more than twelve feet apart. In the Rainbow, the directions of the forces on the four compasses were represented by lines as ↙ or ↙, all included within a small portion of the same quadrant.

The correction of this compass (the tell-tale) was effected by a tentative method, which is likely to be of the highest value in the correction of the compasses of iron ships in general. The ship's head being placed exactly north, as ascertained by a shore compass, a magnet was placed upon the beam from which the compass was suspended, with the direction of its length exactly transverse to the ship's keel: it was moved upon the beam to various distances till the compass pointed correctly, and then it was fixed. Then the ship's head was placed exactly east; and another magnet with its length parallel to the ship's keel was placed upon the same beam, and moved to different distances till the compass pointed correctly, and then it was fixed. The correction for induced magnetism was neglected: but there would have been no difficulty in adjusting it by the same process, placing the vessel's head in azimuth 45°, or 135°, or 225°, or 315°. The peculiar advantage in the method above given, as a tentative method, consists in this: that the magnetic action is resolved into two parts, either of which can be altered without at all disturbing the other, and that by a very simple rule those positions of the ship can be found in which each of these parts is effective while the other is powerless. The same remark applies to the correction for induced magnetism.

The Ironsides has since that time sailed to Pernambuco, and her compasses have been correct (as far as general observation goes) through the voyage. It is probable, therefore, that the constant N is not very large: but no accurate observations have been yet reported to me upon which any certain statement as to that point can be founded.

Section VIII.—*Concluding Remarks.*

It appears from the investigations above, that the deviations of the compass at four stations in the Rainbow, and at two stations in the Ironsides, are undoubtedly caused by two modifications of magnetic power; namely, the independent magnetism of the ship, which retains the same magnitude and the same direction relatively to the ship in all positions of the ship; and the induced magnetism, whose force varies in magnitude and direction while the ship's position is changed. It appears also that, in the instances mentioned, the effect of the former force greatly exceeds that of the latter.

It appears also that experiments and investigations similar to those applied above, are sufficient to obtain with accuracy the constants on which, at any one place, the ship's action upon the horizontal needle depends (namely, $\frac{H}{I\cos\delta} + \tan\delta . N$, $\frac{S}{I\cos\delta}$, M, and P); and that by placing a magnet so that its action will take place in a direction opposite to that which the investigations show to be the direction of the ship's independent magnetic action, and at such a distance that its effect is equal to that of the ship's independent magnetism, and by counteracting the effect of the induced magnetism by means of the induced magnetism of another mass (according to rules which are given), the compass may be made to point exactly as if it were free from disturbance.

It appears also that by an easy tentative method, the compass may now be corrected without the labour of any numerical investigations, or any experiments, except those of merely making the trials.

It appears also that the permanent vertical disturbing force, as far as the examination of the Rainbow authorizes us to draw any distinct conclusion, is not great, and therefore that there is no fear of great disturbance of the compass by the heeling of the ship.

But it appears that one of the magnetic constants consists of two parts, which cannot be separated by experiments on the horizontal magnet at any one place; and that the effect of the impracticability of separating these parts will be, to render the compass incorrect in one magnetic latitude when it has been made correct in a different magnetic latitude (though there is good reason to think that the term on which the variation depends is so small that it may be neglected, except in the case of a ship sailing very near the magnetic pole). And it appears that though in theory the term in question could be determined from observations of the dipping-needle, yet in practice the method fails, because the observations cannot be made with the requisite accuracy. It appears, however, that this term may be determined by observations of the horizontal needle at two places whose magnetic latitudes are different, and that the correction may then be made perfect for all magnetic latitudes.

To these considerations we may add the following: that though the uniformity of the induced magnetism, under similar circumstances, is to be presumed, yet the invariability of the independent magnetism during a course of many years is by no means certain.

These statements suggest the following as rules which it is desirable to pursue in the present infancy of iron-ship building.

1. It appears desirable that every iron sea-going ship should be examined by a competent person, for the accurate determination of the four constants,

$$\frac{H}{I\cos\delta} + \tan\delta \, . \, N, \quad \frac{S}{I\cos\delta}, \quad M, \quad \text{and } P,$$

for each of the compasses in the ship; and that a careful record of these determinations should be preserved, as a magnetic register of the ship.

2. It appears desirable that the same person should examine the vessel at different times, with the view of ascertaining whether either of the constants changes with time.

3. It appears desirable that in the case of vessels going to different magnetic latitudes, the same person should arrange for the examination of the compasses in other places, with a view to the determination of N.

4. It appears desirable that the same person should examine and register the general construction of the ship, the position and circumstances of her building, &c., with the view of ascertaining how far the values of the magnetic constants depend on these circumstances, and in particular to ascertain their connexion with the value of the prejudicial constant M.

5. It appears desirable that the same person should see to the proper application of the correctors, and the proper measures for preserving the permanency* of their magnetism.

The most remarkable result, in a scientific view, from the experiments detailed above, is the great intensity of the permanent magnetism of the malleable iron of which the ship is composed. It appears, however, that almost every plate of rolled iron is intensely magnetic. In the progress of the investigations I have endeavoured to make some experiments with plates of iron, for the purpose of examining the induced magnetism of iron plates in different states of connexion, but they have failed totally, in consequence of the amount of independent magnetism. On placing a compass within a large cylinder of iron plate, whose axis was horizontal and in the magnetic meridian, I was surprised to see that the deviation of the compass was as great as at the binnacle position in the Rainbow.

The scrolls of iron used for correcting the induced magnetism in the Rainbow were repeatedly passed through the fire before their permanent magnetism was destroyed. In some which were tried, even this process was ineffectual. I am disposed to prefer for correctors boxes filled with iron chain; for though many parts of the chain may possess some independent magnetism, yet it is likely that there will be, to a great extent, an intermutual destruction of effects.

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* The magnets which I have fixed in the Rainbow and the Ironsides have been heated to a temperature of 120° Fahr., with their marked ends towards the south, and have afterwards stood vertically with their marked ends upwards for one or two days. For mounting in the ship they have been let into grooves in pieces of wood, in which they have been bedded in tallow.

If we conceive permanent magnetism to depend upon an artificial arrangement of the particles of the metal, the manufacture of rolled iron seems to account in some degree for this amount of magnetism. The iron, after leaving the puddling furnace, is rolled out into bars of considerable length; these are broken, and their pieces are laid side by side, and the united pieces are again rolled out, &c. The whole object of the manufacture is to arrange the particles in that artificial state known by the name *fibre*.

It is believed by practical men that the state of malleable iron changes from time only. If this be certain, and if the notion just mentioned be plausible, it seems sufficiently probable that the independent magnetism of the ship will change with time. This consideration enforces strongly the necessity of periodical examination as suggested above. Such examination may possibly have an advantage beyond the correction of the compass for the time. An important change in the magnetism may indicate an important alteration in the quality of the iron, and may serve as a warning to be cautious of trusting to the strength of the ship in critical circumstances.

I know not whether the axis of magnetism in a plate of iron may be expected probably to coincide with the direction in which it has been extended by rolling. The direction of the horizontal magnetism in the Rainbow (conceiving the transverse part to be produced by the transversal partitions or bulk heads), and the insignificant amount of the vertical magnetism, seem to countenance this supposition. The great difference between the magnetism of the two compasses in the Ironsides may seem, perhaps, to place difficulties in the way of such a supposition.

Royal Observatory, Greenwich,
April 9, 1839.

PHILOSOPHICAL

TRANSACTIONS

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXXXIX.

PART II.

LONDON:
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MDCCCXXXIX.

ADJUDICATION of the MEDALS of the ROYAL SOCIETY for the year 1839 by the PRESIDENT and COUNCIL.

The COPLEY MEDAL to ROBERT BROWN, ESQ., D.C.L., F.R.S., for his discoveries, during a series of years, on the subject of Vegetable Impregnation.

The ROYAL MEDAL, in the department of Astronomy, to JAMES IVORY, ESQ., K.H., F.R.S., for his paper entitled "On the theory of the Astronomical Refractions," published in the Philosophical Transactions for 1838.

The ROYAL MEDAL, in the department of Physiology, including the Natural History of Organized Beings, to MARTIN BARRY, M.D., for his papers entitled "Researches in Embryology," published in the Philosophical Transactions for 1838 and 1839.

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APPENDIX.

Presents . . . . [i]

Meteorological Journal kept at the Apartments of the Royal Society, by order of the President and Council.

Fig 1

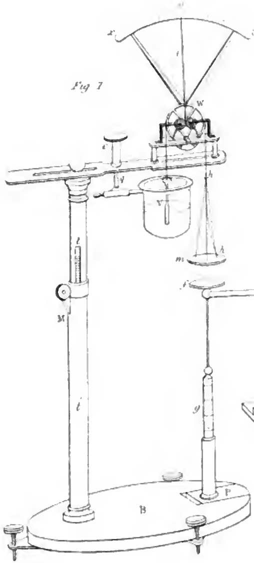


Fig 7

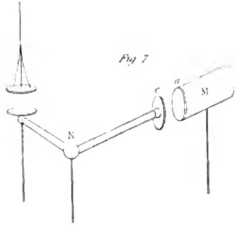


Fig 6

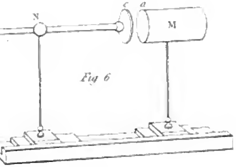


Fig 2

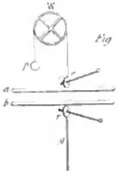


Fig 3

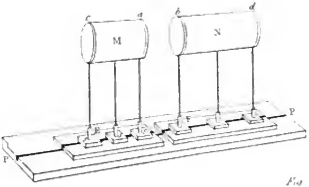


Fig 4

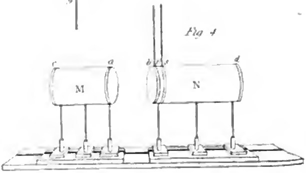
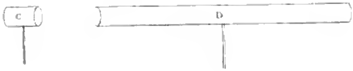


Fig 5



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PHILOSOPHICAL TRANSACTIONS.

XIV. THE BAKERIAN LECTURE.—*Inquiries concerning the Elementary Laws of Electricity.—Third Series.* *By* W. SNOW HARRIS, *Esq. F.R.S.*

Received May 4,—Read June 20, 1839.

1. THERE is no department of science in which the perfection of quantitative measurement, and a clear perception of what we really measure, is more called for than in that of electricity. If we except the valuable researches of Professor ROBISON and of COULOMBE, and the more recent investigations of Dr. FARADAY, we can scarcely be said to possess, in common electricity at least, any connected series of experiments carrying with them a rigid numerical value. In the various inquiries into the elementary laws of electricity, which I have had the honour of submitting to the consideration of the Royal Society, it has been my endeavour to perfect our methods of electrical measurement, whether relating to the quantity of electricity, intensity, inductive power, or other element requiring an exact numerical value, and, by operating with large statical forces both attractive and repulsive, to avoid many sources of error inseparable from the employment of very small quantities of electricity, such as those affecting the delicate balance used by COULOMBE.

The instruments resorted to in these further inquiries have been employed with this view; they have been already described*; I have only occasion to briefly mention some recent improvements in the hydrostatic electrometer mentioned in my first paper*, and represented in Plate III. fig. 1.

2. In the instrument here shown, the column M, carrying the graduated arc, and wheelwork W, consists of two cylindrical brass tubes *t*, '*t*, about an inch in diameter and fourteen inches high. That on which the wheelwork is placed moves freely within the other, so as to be readily elevated or depressed by means of a rack fixed in it, and a pinion attached to the upper part of the outer tube at M. The object of this motion is to enable the experimenter to vary the distance between the attracting or repelling discs *m*, *f* without disturbing the lower disc *f*, or otherwise to adjust the same

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* Philosophical Transactions for 1834.

distance by changing the position of both the discs, manipulations which greatly simplify many intricate cases of experiment.

3. In order to estimate the distances when the position of the disc *m* is varied, a graduated sliding piece *t*, about three inches long, is placed upon the inner tube, free of the rack-work, and being moveable upon it with friction, may be set with any required altitude of the whole column M to zero of its scale. In this way all subsequent changes of distance produced by elevating or depressing the interior tube *t* are easily known.

Changes of distance attendant on the motion of the lower disc *f* are estimated, as before, by the graduated slide *g*, the fixed tube of which is attached to a foot-piece P, moveable in a bevelled groove in the base B; the whole may be hence withdrawn for a certain distance, if required, so as to place the disc *f* without the influence of the upper disc *m*.

The disc *m* is suspended from the fine silver thread passing over the balanced wheel W, by three threads of varnished silk, after the manner of a common scale-pan, so as to insulate it if requisite; it is connected with the ground in ordinary cases by a fine wire, terminating in a small hook loosely hung from the silver thread to the surface of the disc, as at *h h'*, fig. 1.

The glass vessel V containing the water, and counterpoise float, are here supported in a ring of brass, moveable in a brass tube attached to a sliding rod *q*. This rod is acted on by a nut and screw inclosed in a cylindrical piece *c* fixed to the horizontal plate carrying the wheel-work. Hence the water vessel may be elevated or depressed at pleasure, and the index *i* readily adjusted to zero, or any other required point on the arc.

4. These provisions enable us to operate with the instrument in the following way. Let it, for example, be required to estimate the attractive force between the plates *m*, *f* at any given distance, D, suppose ·6 of an inch.

We first bring the discs in contact so nearly as may be, and then set the graduated slider *t* at zero of its scale, by bringing it to coincide with the upper edge of the outer tube M. We then (having also set the slider supporting the insulated disc *f* at zero) either raise the tube *t* ·6 of an inch, or depress the slide *g* by the same quantity, or otherwise raise the upper and depress the lower disc by quantities making together ·6 of an inch; in either case the discs will be finally ·6 of an inch apart, measured between the opposed surfaces previously in contact. Under these conditions let either plate be taken insulated and charged, whilst the other is neutral and free. Suppose the lower disc *f* to be charged with a given quantity, and the suspended disc *m* free. Then the attractive force which ensues will cause the index to advance in the direction *o y* a given number of degrees, consequently the distances between the plates *m*, *f* will be diminished. Let the index be now brought again to zero by turning the milled head of the screw *c* so as to depress the water vessel V; then the force between the plates, whatever it be, is acting at ·6 of an inch. To discover the amount of this in degrees, we discharge effectually the air and opposed plates *m*, *f* by touching them simultaneously with a bent wire. The force then vanishes, and the index declines in the direction *o x*. The amount of this declination is evidently the force in degrees at the given distance $D = \cdot 6$ of an inch.

Do we require to obtain a force of any given number of degrees at a given distance, we first adjust the distance as before, the index being at zero of the arc, then depress the water vessel V until the index has declined the given number of degrees in the direction *o x*; we now continue to add small quantities of electricity to the insulated disc *f* until the index is again brought to zero, we have then in noting the quantity of electricity the required attractive force due to this quantity at the given distance*.

5. The general principles of this instrument, together with its mode of action, being now understood, I may merely observe, that although not available for the measurement of such minute forces as those applicable to the balance of torsion employed by COULOMBE, it is still peculiarly delicate, and admirably well adapted to researches in statical electricity. Its indications depending on the force between two opposed planes operating on each other under given conditions, are reducible to simple laws, and are hence invariable and certain. The attractive force between the discs is not subject to any oblique action, is referable to any given distance, and may be estimated in terms of a known standard of weight.

6. It may not be unimportant to consider here the nature of the indications of this and similar instruments in electricity; in other words, what we really measure by means of such instruments.

This question is of considerable moment; for should the force between the discs be in any way influenced by a precarious distribution of the electricity, on or about their surfaces, and liable to become further complicated by the distribution on the surfaces of the conductors, and other bodies placed occasionally in tortuous connection with them, as in fig. 7, 8, 9, it would, perhaps, be impossible to say what we did really measure by any instrument such as that described; we should at least require the aid of a formidable analysis involving definite integrals of unknown functions to determine it: fortunately however such is not the case; the force between the attracting plates *m*, *f*, stands, as may be clearly shown by experiment, quite clear of any hypothetical distribution whatever of the electricity, with which the discs are charged, or with which any series of bodies may be charged in connection with them. Thus the general laws of electrical action arrived at in my former communications† remain uninfluenced by any new condition of the connecting rods, or other bodies of variable form, with which the attracting bodies may happen to be connected. The question, therefore, what do we measure? is very easily answered, as readily, in fact, as a similar question would be, if applied to the thermometer or any other instrument, the nature of whose indications are necessarily determined by experiment. Thus we observe that with double, treble, &c. quantities of electricity, either accumulated on the insulated plate *f*, or on any insulated conductor connected with it, we have invariably 4, 9, &c. times the attractive force indicated on the graduated arc, the distance between the discs remaining the same; no matter for the form of the charged bodies in connexion with the insulated plate *f*, or the form or disposition of any number of rods connecting them. Hence conversely, when the attractive forces are at distance unity, 4, 9, &c. times as great, then the respective quantities of electricity accumulated, under the existing disposition of the conducting surface, are 2, 3, &c. times as great; the quantity being as the square root of the indicated force. Similar observations apply to a variety of other quantitative measurements, of which this instrument is susceptible, as in the case of the attractive force communicated to the insulated plate *f*, when connected with a charged conductor of any given form, and which by experiment remains invariable, at whatever point of it the connection be made. Hence the general electrical intensity of such a conductor is always represented under any new disposition of its surface, as shown in my experiments on the capacity of rectangular plates, cylinders, spheres[*], &c. These and similar facts deducible by experiment enable us, independently of all theory, to investigate by this electrometer certain electrical relations, such as that of quantity to surface, of intensity to figure, and the like, with accuracy and precision. This question then being disposed of, we may proceed to the further consideration of the elementary laws of electrical action, the subject of these inquiries.

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* There are some precautions essential to observe in the use of this and similar instruments, which will be apparent as we proceed.

† Transactions of the Royal Society for 1834.

7. Electrical attraction and repulsion, as commonly observed, are invariably attended, if not altogether preceded by other forces of a more elementary character, without the presence of which, neither of these interesting phenomena would probably ensue. These more primary actions we have, in accordance with the prevailing theories of electricity, classed under the general head of inductive actions. **Faraday** has lately investigated the nature of these actions in the Eleventh Series of his admirable Researches in Electricity. The three following experiments exhibit some phenomena of induction not generally noticed, although the results are such as might be previously anticipated: the experiments however are still new of their kind, and very illustrative of what takes place, at the instant two bodies attract or repel each other by the agency of electricity.

Let a circular disc of gilded wood, about six inches in diameter, *a*, fig. 2, be suspended by an insulating thread of varnished silk from a common balance or the periphery of a light wheel W, the axis of which rests on friction wheels so as to allow it great freedom of motion; attach a delicate electroscope *r*, to this disc, and counterpoise the whole by a weight *p*. Let a similar disc *b*, insulated on the glass rod *g*, and having also an electroscope *r*ˋ attached to it, be placed at any convenient distance immediately under the former*: when the two discs are placed in certain electrical relations to each other, under a good insulating state of the air, the following phenomena may then be observed.

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[*] Philosophical Transactions for 1834.

Exp. 1. The disc *b* being charged with either electricity, and opposed to *a*, insulated and neutral, we observe the electroscope *r* of the neutral disc begin to rise off its surface, whilst that of the charged disc *b*, already in a state of divergence, will tend to collapse. When these respective effects ensue, the suspended disc descends towards the charged disc.

Exp. 2. The two discs being both previously charged with opposite electricities, we observe, in opposing them as before, both the electroscopes, *r*, *r*', begin to fall back, at which instant the discs appear to attract each other as before.

Exp. 3. The discs being now both charged with the same kind of electricity, we observe the divergence of the electroscopes *r*, *r*' increase; at this instant the suspended disc recedes from the fixed disc, being apparently repelled by it.

It may be further observed in the first experiment, that the operation of the neutral on the charged disc, as indicated by the electroscope *r*', is greater when its superficial extent is increased. Thus, if it have a temporary connexion with an insulated conductor of twice or three times its surface; or otherwise, if it form the terminating surface of a hollow cylinder of the same diameter and of any given altitude, the collapsing of the excited electroscope *r*' attached to the charged disc will be more considerable. If it have a conducting communication with the ground, then, as is well known, its influence on the charged disc is, at a constant distance, the greatest possible. In many of these cases, however, the influence of the charged on the neutral disc is, on the contrary, less sensibly shown by the electroscope *r* attached to the latter, in consequence of the operation being extended to larger masses, and to more distant points.

8. The respective influences on the electroscopes just observed, and which precede, or *at least* accompany the attractive or repulsive forces between the discs, may be distinguished in the following way. In the case of the first experiment, the electroscopes indicate two inductive and simultaneous actions. One of these may be considered as a direct induction, the other as a reflected induction; supposing, as is not unlikely, that the induction of the neutral on the charged plate is entirely dependent on the electrical state excited in the former by the direct influence of the latter, by which an opposite force is induced, and which reacting on the charged disc, neutralizes a portion of its free electricity.

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* The electroscopes *r*, *r*' may consist of light reeds, with pith balls at their extremities, counterpoised on a delicate axis of brass, set on points in metallic rings *r*, *r*'. These are fixed in the centre of the plates *a*, *b*. The reed and ball on the upper one is a little heavier than the counterpoise *r*; on the lower one the counterpoise is heavier than the reed and ball. Thus, in a neutral state, the balls lie parallel with the surfaces of the plates, and rise from them by repulsion, with the slightest force. It is only requisite to observe, that the pith balls should not in the neutral state actually *touch* the surface, but remain elevated within a very small distance of it, which may be readily effected by fixing a small slip of cork to the plate just under the reed. By this we avoid any adhesion of the ball to the plate, which interferes with the success of the experiment.

9. Pursuing this view we may further infer, as in fact may be shown experimentally, that similar inductions really tend to arise in both the subsequent experiments 2, 3, notwithstanding the presence of the similar or dissimilar electricities; that is to say, inductive forces tend to arise in each disc, similar to those which would ensue if either plate were reduced to a state of neutrality, thus increasing the amount of the attractive forces in the case of the plates being charged with opposite electricities, and decreasing that of the repulsive forces when the discs are charged with similar electricities*: the laws of electrical attraction and repulsion therefore are intimately associated with these inductive forces; hence, as observed by FARADAY, it is requisite to examine rigidly the nature and operation of this wonderful influence.

10. There are many striking phenomena of electrical induction, which lead us to conclude that it is in some way dependent on the presence of an exquisitely subtle form of matter pervading bodies, which may become disturbed in them, and assume new states or conditions of distribution. The following experimental analysis goes far to place this beyond the limit of a mere hypothesis, and to confirm it as an elementary principle in electricity.

M, N, fig. 3, are two cylinders of gilded wood, about four inches in diameter and six inches long; the extremities of these cylinders terminate in thin slices, *a*, *b*, *c*, *d*; all the different pieces are insulated on slender rods of varnished glass, fixed in separate stands; these slide upon the pieces E, F by means of a groove and steadying pins in the stands; thus the false ends *a*, *b*, *c*, *d* may be either placed in contact with the cylinders M, N, or be otherwise placed at any given distance from them. The pieces E, F also slide in a similar way upon the base P P, so as to admit of the two bodies, M, N, being placed at any given distance apart.

Exp. 4. Electrify the cylinder M, and slide it to within any given distance of N; the latter will, as is well known, become electrified by induction, and the cylindrical slices *b*, *d*, if removed on their insulating rods, will be in opposite electrical states. But if before removal the charged body M be withdrawn, then the whole system returns to its previous state, and exhibits no electrical sign whatever.

Now it may be inferred, that if the peculiar condition of the extremities *b* or *d*, considered as portions of the body under induction, depended merely upon some peculiar affection of the particles of common matter, and not on some agency associated with them, then on removing the slices *b*, *d* forming the extremities, the forced state should vanish; for it is difficult to conceive how any principle of return to quiescence applicable to the whole body N, when removed from the influence of M, should not also apply to any part of it placed under the same conditions, *e. g.* to the thin sections, *b*, *d*, forming its extremities. But on removing the extremity *d* we find, as just stated, that the forced state remains.

Exp. 5. Examine in a similar way the thin slices *a*, *c*, constituting the extremities of the charged body M, having first determined the intensity of the charge previously to opposing it to the neutral cylinder N. Then we find the intensity of the distant extremity *c* considerably diminished, and that of the proximate extremity *a* considerably increased; and this effect becomes the more apparent as the distance between the bodies is less, and will be strikingly shown if N be connceted with the earth*.

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* Philosophical Transactions for 1836.

Exp. 6. Electrify the cylinders M, N, one positively, the other negatively; then on examining each as before, similar results will ensue; the distant extremities will show on removal less accumulation than the proximate ones.

Exp. 7. Electrify the cylinders, both positively, or both negatively, we have then an opposite result. The distant extremities will exhibit a higher positive or negative accumulation than the near ones.

11. These results, therefore, all appear to show, that some extremely subtle form of matter pervades bodies, which may be caused to change its state in them in respect of quantity. To determine the respective states of accumulative change in the bodies, I employed the electrometer just described, and examined them, either by their inductive influence on an area of equal magnitude, or by simply opposing the removed slices to the suspended plate *m*, fig. 1. at a constant distance, or by other methods not necessary to dwell on here. I obtained in this way some interesting results in respect of the relative quantities of electricity displaced at given distances, which will be noticed in another place (32.).

If we place at the extremity *b*, of the cylinder N, three or more consecutive insulated slices, insulating them on glass rods, in any convenient way as in fig. 4., and then proceed to examine the electrical state of these slices whilst under the influence of the charged body, the electricity of the distant extremity *d*, will frequently be found extending up to the last section *b*; and, contrary perhaps to what we might expect, the point to which the electricity of the distant extremity *d* extends toward *b*, will be greater as the intensity of M is greater and its distance from N less, as if the displaced electricity not being enabled to pass freely off in the direction *b d*, supposing M positively charged, was continually, as it were, bounding back or reverberating upon the extremity *b*, a fact which may be further observed in the following experiment.

Exp. 8. Oppose a cylindrical conductor D, fig. 5., about three feet in length and four inches in diameter, to an electrified cylinder C, charged with positive electricity. Test the state of different points of this cylinder D by touching it with a tangent disc of very small thickness. If we ascertain in this way the first point at which the electricity evinced is negative, we shall find on bringing the charged cylinder C within a less distance, or otherwise increasing its intensity, that the same point will become positive. The same thing occurs in increasing the extent of the opposed areas at the extremities of the cylinders, until at last the points immediately in the vicinity of the opposed end will become positive.

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* In these experiments it is better to detach first the central portions M or N whilst under induction, when we wish to examine the state of the remaining slices *a*, *b* or *c*, *d* forming the extremities.

Exp. 9. Detach in fig. 4. the central part N and extremity *d* of the cylinder N, and remove the charged body M. Then if the extremity *b* consist of two or more sections, *b*, *r*, *s*, these sections will evince negative electricity, M being positively charged; but if after detaching the central mass N we allow the charged body M to remain, and then take away the further slice *s*, this slice, which under the former arrangement evinced negative electricity, will now evince positive. There must consequently still remain a portion of electricity sufficient to electrify the remote section positively, although not equivalent to the negative state of the three slices taken together, and removed from without the influence of the charged body.

12. Since then the negative state of an indefinitely thin slice *b*, immediately opposed to the charged body, may be supposed to depend on the electricity displaced from it, and collected in the detached central mass N, we may conclude, in accordance with the well-known fact, that this negative state will be the greatest possible when the electrical capacity of N is indefinitely great, that is to say, when it is connected with the ground, and whilst the influence of the charged body M is still operating on it. The near surface *b*, therefore, of a free neutral body, N, fig. 3, under the induction of a body, M, positively charged, is greater than would be apparent upon the whole body after cutting off its connexion with the earth.

The inductive action, whatever it be, which thus takes place between a charged and neutral body, does not appear to be in any degree influenced by angular divergence, but is exerted equally in every direction from the point at which the induction first commences; this may be inferred from the following experiment.

Exp. 10. A cylindrical conductor, N, figs. 6, 7, was so constructed as either to constitute a single straight line, fig. 6, or assume a rectangular form, as in fig. 7. It consisted of two straight cylinders of gilded wood, each about a foot in length, and three-fourths of an inch in diameter, united to an intervening ball N by means of short brass pegs. In this way the two cylinders could be easily placed in one straight line, as in fig. 6, or at right angles to each other, as in fig. 7. Under this condition one extremity, *f*, fig. 6, was placed in contact with the insulated ball *f* and disc of the electrometer, fig. 1, and the opposite extremity united to a light disc *c*, of about three inches diameter. The inductive force was impressed upon this last by a charged cylinder, M. The distance, *c a*, and quantity with which M was charged being the same, it was easy to discover whether any difference arose in the attractive force on the electrometer at the extremity *f* when the conductor was bent at right angles, as in fig. 7, or otherwise allowed to form a straight line, as in fig. 6. No appreciable difference, however, was observable in these different dispositions of the conductor N. Thus the distance *c a* being one-fourth of an inch, and the charge the same, the electrometer indicated in each instance ten degrees, the discs *m*, *f* being also one-fourth of an inch apart. The quantity and distance *c a* was varied, but a similar result ensued. It may be hence inferred that the force impressed upon the disc *c* is propa-

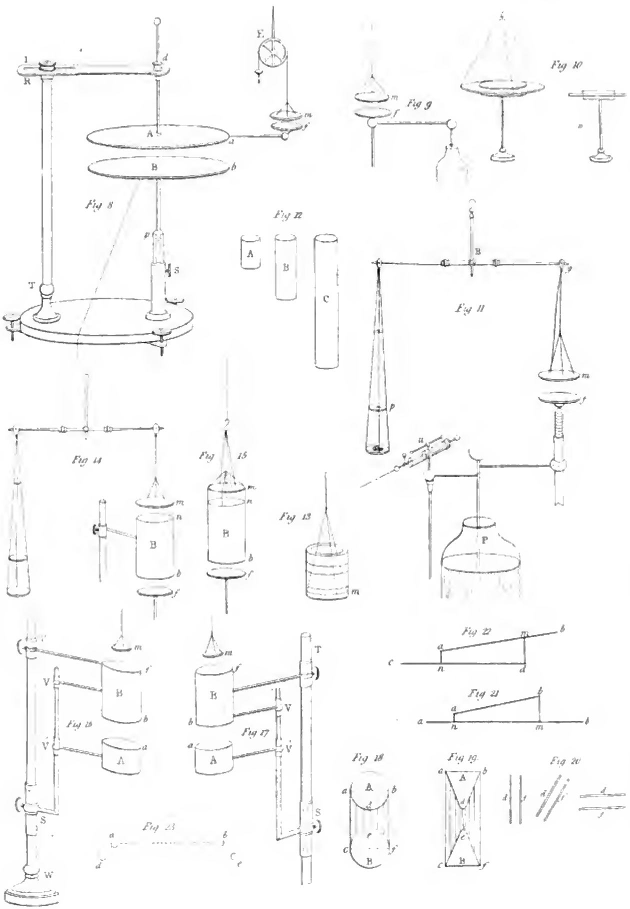
Fig 8
I
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Fig 9
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Fig 10
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Fig 12
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Fig 11
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g
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m
f
u
P
Fig 14
m
n
B
b
f
Fig 15
m
n
B
b
f
Fig 13
m
Fig 16
T
V
B
f
b
V
A
a
S
W
m
Fig 17
m
f
B
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T
V
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Fig 22
a
m
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Fig 21
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Fig 23
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c
e
Fig 18
A
a
b
d
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c
B
f
Fig 19
a
b
A
d
e
c
B
f
Fig 20
d
f
d
f
d
f

gated in the conductor equally in all directions, thus exhibiting in a remarkable way one of the primary laws of fluids.

13. Since these facts furnish some evidence in favour of the conclusion that electrical phenomena result from certain changes in the distribution of a subtle kind of matter associated with the particles of bodies, it may not be unimportant to examine the laws of these changes in the cases of induction and attraction above mentioned.

It must be apparent by experiments 4, 5, 6, figs. 3, 4, that in the ordinary attractive force between a body positively charged with electricity, and a neutral body N in a free state, three actions arise claiming particular notice, viz. a receding of the natural electricity of the neutral body, from the points nearest the body charged positively; a passing of electricity from the remote points of the positively charged body toward the neutral body; lastly, a tendency of the opposite electrical forces to come together and enter into a species of union; which last condition seems to be the immediate cause of these two bodies approaching each other, all impediment to motion being removed.

14. With a view of discovering some of the laws of these induced changes, I resorted to the method represented in Plate IV. fig. 8, in which A, B are two flat discs of wood covered with tin foil, insulated on varnished glass rods, A *d*, B *p*. The upper disc A is connected with the insulated disc *f* of the electrometer E, and is supported by the arm R *d* fixed to the glass or wood column R T. The lower disc B can be set at any required distance from A by the graduated slider S, and retained there by a stop screw, S. The rod *d* A also is moveable with friction through a compressed collar of cork at *d*, by which it can be elevated or depressed for the more perfect adjustment of the contact of the plates A, B, when the slide S is at zero of its scale.

Exp. 11. The distance, *a b*, between the plates being made = ·3 of an inch, and the plate B connected with the earth, a charge was accumulated on A, indicating on the electrometer two degrees, the discs *m*, *f* being set at ·4. The plate B was now depressed to twice the distance, so as to make the distance *a b* = ·6 of an inch. The electrometer now indicated at the same distance of ·4, eight degrees. By making *a b* = ·9, or three times the first distance, the electrometer had advanced to eighteen degrees. The march of the electrometer therefore was directly as the squares of the distances between the plates.

15. Now it is clear in this experiment that the quantities of free electricity in the upper plate were inversely proportional to the distances between the opposed surfaces, the quantity by the demonstrated law of the electrometer (4.) being as the square root of the indicated attraction; hence the force or influence of the lower plate, that is the reflected induction (8.), is, if measured by the quantities of electricity it ceases to hold in equilibrio, as the distance *a b* between the plates A, B inversely; if measured by the indication of the electrometer, inversely as the squares of this distance.

16. Exp. 12. The former experiment relates to the influence of distance; it may

not be unimportant therefore to observe the effect of quantity under similar circumstances. In this experiment, then, the distance *a b* was first fixed at ·4 of an inch, and four measures of electricity deposited on the plate A. The indicated force with this quantity amounted to 3°. When eight charges were placed on A, that is double the former, the indicated force amounted to 12°, or four times the former, and so on in the same ratio of the square of the number of charges, up to the limit of the action of the electrometer, which is the law above mentioned, and shown for the coated jar in my first series of papers[*].

17. I confirmed this result by making the distance $a\ b = \cdot 8$, and communicating to the insulated plate the same number of charges. The electrometer now indicated 46°, being little different from 48°, the number which should appear by the former experiment. The plates and electrometer discs were now perfectly discharged, and the same eight charges deposited on A when the distance *a b* was reduced to ·2 or one quarter. The electrometer now indicated 3° only, or $\frac{1}{16}$th of the former force.

18. The reflected influence of the lower plate therefore is such, as to hold quantities of electricity in equilibrio directly proportional to the quantity with which the insulated body is charged.

Exp. 13. I extended these experiments to opposed plates whose areas were less than the former, but equal, and found, as in the former experiments with the Leyden jar, that the quantity and distance being constant, the indicated force was as the squares of the opposed areas inversely. Thus, when the areas of the plates were doubled, the force was only one-fourth as great.

19. We cannot by this method deduce any accurate result for plates of unequal area, as in my former experiments on attraction[†], since the charge expanding over the whole of the upper disc, and also the electrometer, it does not admit of being neutralized throughout, hence the influence of the opposed portions only is not apparent; neither will the laws above mentioned be rigidly exact, if we charge the insulated plate A without the influence of the opposed plate B, since in this case also the electrometer discs receive a maximum of charge at once, which cannot be subsequently diminished so as to show the action of the neutral plate B. But in accumulating the charge under the influence of B, the electrometer charges gradually, and with the electricity not held in equilibrio by the lower plate.

20. In the three preceding experiments the neutral disc B, had a free connexion with the ground. We shall now take it insulated.

Exp. 14. The plate A being charged under the influence of B as before, at a distance of ·2 of an inch, indicated by 4° of the electrometer with the discs *m, f* at ·5, the conducting connexion of B with the ground was removed, so as to insulate it. In this case the induction upon B was the greatest possible at the given distance ·2. The plate B was now depressed ·2 and ·4 of an inch further, so as to obtain double and quadruple the first distance successively. The march of the electrometer in this case was no longer as the squares of the distances *a b*, as in Exp. 11, being now as the distance; thus at ·2 the force was 4°, at ·4 = 8°, at ·8 = 16°.

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[*] Transactions of the Royal Society for 1834.

[†] Ibid.

The following table exhibits the results of the comparison of the insulated and uninsulated state of the plate B at other distances and with other charges.

Table I.

Insulated at ·3.		Uninsulated.	
Dist.	Force.	Dist.	Force.
·3	3°	·3	3°
·6	7	·6	12
·9	10	·9	26

The approximation to the laws above mentioned are here very close.

It is then apparent from these results, that by limiting the electrical capacity of the opposed plate, we fix the direct inductive action up to a certain limit; hence the subsequent effect is due to change of distance only, and is in a simple inverse ratio of the distance. Faraday has shown in the Eleventh Series of his valuable Researches in Electricity, since this experiment was first made, in what this effect of change of distance consists; he observes, (sec. 1303,) "there is perhaps no distance so great that induction cannot take place through it: but with the same constraining force, it takes place more easily, according as the extent of the dielectric is lessened," &c. &c.

21. Exp. 15. A plate of glass very dry and varnished being opposed to the plate A, and a charge accumulated as before, no change was perceptible on altering the distance, or on the removal of the glass; we may hence infer, that a perfectly non-conducting substance is insensible of any new electrical state, by simple induction under common circumstances.

Exp. 16. The plate B being insulated and opposed to A, the difference on the electrometer after removal was still very small, and at moderate distances of ·2, ·3 of an inch quite inappreciable. These differences, however, became greater by increasing the thickness of the plate, or by allowing a small conducting rod to project from it.

We may, therefore, further infer, that in an insulated conducting disc indefinitely thin, the inductive capacity is indefinitely diminished*.

22. Electrical attraction then, is evidently a complicated operation, and may not unfrequently give rise to results apparently of an anomalous character. The uniformity of the force depending on a perfect accomplishment of the inductive changes above mentioned, should these be in any way limited, or interfered with, the force may appear to vary, or be also limited in the law of its action to certain distances, as is apparent in the following experiments.

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* The case must not be taken as identical with that described by Faraday (1295.), we are here speaking of an insulated disc, not a disc connected with the earth: no sensible thickness is then required, as already shown by the suspended disc of the electrometer.

Exp. 17. When two circular discs *m f*, fig. 1, were opposed to each other, the suspended disc *m* being perfectly insulated and thin, and the lower disc charged positively, little or no attractive force was observable at ·3 of an inch distance. At ·2 of an inch the force amounted to about a degree. On touching the neutral disc with a conducting rod the force appeared to be indefinitely increased, not being under the same charge and distance measurable by the instrument.

Exp. 18. A varnished disc of glass being substituted for the disc *m*, the force was not appreciable at any distance.

Exp. 19. The opposed discs being charged, one positively, the other negatively, and to as nearly the same degree as possible, the forces were observed corresponding to various distances by the process above described (4.); they were as the simple distance inversely, or very nearly so. Now it is to be observed in this case, that the discs being thin, the positive and negative accumulations, as in Exp. 14., were already the greatest possible, or very nearly so: all subsequent inductive change was therefore precluded, and hence the increase of force was due to change of distance only, or according to FARADAY, to the intervening dielectric particles being lessened.

Exp. 20. The lower disc being charged positively and the upper plate allowed to remain neutral and free (4.), the force within given limits was as $\frac{1}{D^2}$, but at near distances as $\frac{1}{D}$.

In this case the lower disc *f* having a limited thickness and charge, is not fully susceptible of the reflected induction at all distances; hence this force is impeded as before, and the conditions at last approximate to those of the permanent positive and negative states in the preceding experiment.

Exp. 21. The lower disc *f* being connected with a charged jar or coated plate either of air or glass, as in figs. 8 and 9, and the suspended disc *m* placed in connexion with the ground, the forces were as the squares of the distances inversely at all the distances at which the experiment could be tried.

It is to be observed here, that the inductive capacity of each surface was indefinitely increased, whilst the quantity of electricity accumulated might also be considered as indefinitely great in respect of the charged surface.

23. The generally received law of electrical attraction would, by these phenomena, appear to be rather a result of the conjoint operation of two elementary actions than a simple law, such as that observable in the action of forces supposed to emanate from a centre, since it is not demonstrable, except under given conditions of induction peculiar to the attracting bodies. When the positive and negative states are fixed and invariable, the attraction between the plates, as found by experiment, is really in a simple inverse ratio of the distance.

Exp. 22. With a view of examining this result more rigidly, and disengaging the electrical particles so far as possible from all association with a conducting substance, I procured two thin circular plates of glass, about ·6 of an inch diameter, and having given them temporary coatings of gilded wood, as in fig. 10, I charged each with a given quantity. The coatings were now removed, and the charged glass plates transferred to the electrometer with the positive and negative surfaces opposed to each other, as at *m f*, fig. 1. The lower plate in this case was supported on a slightly curved glass, similar to a common watch-glass, so as to avoid the presence of any conducting substance, and three silk lines of suspension attached to the upper plate by a little sealing-wax. In this experiment we may conceive the force to result purely from the action of the opposite electricities, which may in this case be considered as fixed and incapable of any further change, since by the law of the coated jar no electricity can be added or taken from one side without a simultaneous corresponding change on the other, hence one side only may exhibit free electricity. The glass plates themselves also, not being susceptible of induction (Exp. 15.), cannot be supposed to share in the attractive force, whilst the remote surfaces of each plate are virtually neutral. Under these circumstances the force varied very rigidly as the distances between the plates inversely, at all distances at which the experiment could be tried.

24. The relations of the inductive to the attractive force becomes under this view an interesting subject of inquiry in electricity. I endeavoured to examine still further by a careful series of experiments the general laws of these forces, and succeeded in arriving at many results calculated to throw additional light on the nature of electrical action.

Two discs *m*, *f*, fig. 11., were opposed to each other, as explained in the Society's Transactions for 1834, p. 220, that is to say, the disc *m* was suspended from one arm of the balance B, whilst the disc *f* was connected with a coated jar; the attractive force between the discs at various distances being measured by weights placed in the scale pan *p*, and the quantity of electricity estimated by the unit jar *u*.

Exp. 23. The suspended disc being very perfectly insulated by varnished silk lines, the force of attraction at a constant distance was examined with a given quantity of electricity in a very dry atmosphere, and subsequently compared with the force under the same conditions of quantity and distance, when placed in a free state by a small connecting wire accurately balanced and hung from the point *g*; the difference, as may be anticipated, was very great. In the insulated state, it required sixty charges before the force was equal to one grain at a distance of ·2 of an inch. In the free state, three charges only were requisite to raise one grain at ·2.

Now we have before seen, that the force between two attracting discs, is as the square of the quantity of electricity communicated to the charged body; the force, therefore, in the free state with sixty charges, could this quantity be accumulated and retained, would have amounted to 400 grains; or otherwise taking only three charges in the insulated state, it would have been only the $\frac{1}{400}$th of a grain. The force therefore in these two states may be taken as inversely proportional to the square of the number of charges. Hence the force between the plates was greater in the free state in this particular case, in the ratio of 400 : 1.

25. Exp. 24. With a view of observing the rate of increase of the attractive force as the capacity of the neutral body was caused to increase, I placed successively on the suspended disc *m* a series of rings of gilded wood, as in fig. 13, so as to increase the thickness from ·1 of an inch to two inches: the results are given in the following Table.

TABLE II.

Thickness	·1	·2	·4	·6	·9	1·2	2·0
Charges	60	50	40	30	24	20	16
Force compared with the force when free	400 : 1	277 : 1	177 : 1	100 : 1	64 : 1	44 : 1	28 : 1
Force of sixty measures reduced to grains	1—	1·44	2·25	4	6·2	9	14

The amount of charge for the first experiment, both in the insulated and free state, was as near as could be determined. The beam dropped in the latter case with something more than three charges, hence the force must be assumed as something less than a grain. In the insulated state it was not easy to arrive at the precise number of charges, although the variations were not considerable: sixty charges of the unit measure, however, corresponded upon a mean number of experiments, to the same force, about one grain. If we assume the force for thickness ·1 to be something less than a grain, let it for example be taken at ·7, then the force as expressed in the lower line would increase nearly as the thickness or altitude of the cylinder, which is not a little remarkable.

Exp. 25. I followed out this result by examining the force upon three cylindrical conductors, A, B, C, fig. 12, whose altitudes were 1·5 inches, three inches, and six inches, that is, as the numbers 1, 2, 4, and having terminating plane surfaces equal to that of the plane disc *m*, fig. 11. These being suspended successively from the balance, the force, taken in a free state, amounted as before to one grain, with rather more than three charges, being the same as the simple disc taken in a free state; when taken insulated, the forces varied, and were nearly as the altitude of the cylinders directly. Now on referring to the induced force, I found that all this time the inductive charges upon the disc and cylinders continually increased with the thickness. Thus when the two extremely thin slices, *b*, *r*, fig. 4, were employed alone, and the experiment taken as before in Experiment 4, the opposite electrical state of the near slice was extremely small; whereas on increasing the number of slices, and finally the extent of the body N, the induction continued to increase rapidly, and nearly in proportion to the length of N, up to a certain limit.

26. The attractive forces with these cylinders taken insulated, were found to be as the square of the number of charges accumulated, and inversely as the distance,—a result I subsequently confirmed, and found general in all cases of attractive force

between a charged and insulated neutral body. The following Table, abridged from experiments too numerous to detail here, exhibits numerical examples of this result, the charges and distances being given for forces of 1, 4, &c. grains.

Table III.

Distance of surfaces.	A. 1·5 inches.		B. 3 inches.		C. 6 inches.	
	Charges.	Force.	Charges.	Force.	Charges.	Force.
⎧ ·2	20	1	15 —	1	9 +	1
⎩ ·2	40	4	30 —	4	19	4
·4	40	2	29	2	18 +	2

It may be here seen that the force is as the square of the quantity divided by the distance, and may be hence represented by the general expression $F = \frac{Q^2}{D}$. We may further, by this result, deduce the force which would arise, in each case, with a unit of quantity at a unit of distance, and hence arrive at the comparative force for each altitude, A, B, C. Thus in the first line, if we take twenty charges as a unit of quantity throughout for A, B, C, we obtain, taking as before the force which would arise as the square of the number of charges (4.), the following result very nearly:—

Cylinder A.	1·5 inches high.	Force 1.
Cylinder B.	3 inches high.	Force 2.
Cylinder C.	6 inches high.	Force 4.

By which we perceive that the forces are as the altitudes, or very nearly, the approximations being evidently very near, thus confirming the result already arrived at (25.). It is not unlikely that in this case we remove, by increasing the length of the cylinder, the similar electricity to a greater distance from the charged body; and hence the force between the dissimilar electricities is less disturbed, so that we may in this case be merely measuring the difference between the attraction of opposite electricities and the repulsion of similar ones.

27. Exp. 26. I endeavoured to observe the relation between the attractive and inductive forces more directly in the following way. Having interposed a cylindrical conductor, B, fig. 14, about three inches high, between the charged plate *f* and the suspended plate *m*, and noted the distances *n m* and *b f*, the number of charges was determined corresponding to a given induced force in B, as measured by the effect on *m*. The intermediate cylinder was now attached to the suspended disc *m* by varnished silk lines, as in fig. 15, and both suspended from the balance, so as to ascertain under precisely the same conditions of distances, *m n*, *b f*, and quantity of charge, the amount of the attraction between *b* and *f*.

By this method we obtain, 1° a measure of the induction, 2° of the corresponding attraction; and may hence compare these forces under the same or certain relative states; the distances *b f* and *m n* being either constant or variable. I examined in this way the relative forces of induction and attraction in a great variety of cases, and obtained the following general result, viz. the attractive and induced force was either precisely the same or otherwise in the same ratio, or otherwise reducible by the application of the general laws above mentioned (4.) to the same numerical value. The following Table contains the respective forces of attraction and induction, at given distances, &c. between the opposed bodies *b*, *f*, which may perhaps be quite sufficient as an experimental illustration.

TABLE IV.

Distances.		Induction.		Attraction.	
Charged plate. Dist. *b f.*	Suspended disc. Dist. *m n.*	Quantity.	Force.	Quantity.	Force.
A. ·2	·2	9−	1	9	1
		17	4	17+	4
		27	9	26	9
B. ·2	·4	19+	1	10	1
		40	4	21	4
		60	9	32	9

28. We perceive in this Table that the first forces (A.) of attraction and induction correspond to the same quantity of electricity, or very nearly, and are therefore to be considered the same. In the second set of forces (B), where the distance of the suspended disc *m*, by which the induction is estimated, is increased, the number of charges corresponding to the inductive force is greater. Still the forces of induction and attraction are in the same ratio, as compared with the quantity of electricity. Thus we have, nearly,

Ind. quant. 19 + : Att. quant. 10 : : Ind. quant. 40 : Att. quant. 21

or

Ind. quant. 40 : Att. quant. 21 : : Ind. quant. 60 : Att. quant. 32.

But in comparing these numerical values we may reason thus: since the quantity and distance for the forces (B) differ, let them be reduced by calculation to a unit of distance and a unit of quantity; let the unit of distance be ·2, and let the unit of quantity be about nine or ten charges, as in the first line of the forces A; then taking quantity 19 + corresponding to inductive force 1, we should have for 9·5 charges, that is, half the number of charges, only one-fourth of a grain (4.); but in reducing the distance ·4 also to one half or ·2, this force would be again quadrupled, and would become one grain as before, supposing the force on the suspended disc *m* to vary as the square of the distance inversely, which by experiment it was found to do in this case sufficiently near. If we pursue the same course with the remaining experiments on the inductive forces (B), a similar result will be arrived at. Thus we may reduce the forty charges to twenty-one, the quantity for the attraction, and take the distance ·2 instead of ·4, we have then similar forces of four grains. The real state of this and similar cases, is simply this. In consequence of the increased distance of the suspended plate, twice the number of charges accumulate before the same induced force is shown by the electrometer, although the induction is really doubled; hence in suspending B, fig. 15, the attraction takes place with half the number of charges, that is, half the induction, being the same as before (A).

29. These experiments require precision in the adjustment of the respective distances, and in the measurement of the respective quantities; the approximations may therefore be considered as being very close, especially when we take into the account the many delicate manipulations of a general character peculiar to electrical experiments. It is, however, easy to discover when the result is disturbed by errors of observation, or by accidental variations in some of the circumstances attendant on a long series of experiments. Thus in the preceding Table (B) it is quite apparent that the number of charges corresponding to the induced force would be double of those corresponding to the attractive force; and that the difference of a few sparks of the unit measure arises probably from some minute variation in some of the conditions of the experiment. I tabulated many hundred results; some of them were perfectly coincident and exact in the numerical values above given, others less so; but I had no difficulty in observing the laws above mentioned throughout.

30. It may not be amiss to observe, that, as a preliminary step, the influence of the upper disc *m*, fig. 14, was examined experimentally, since the active inductive force may be supposed to proceed more easily in proportion as this disc in a free state is placed nearer to the body B under induction. I had not, however, much trouble in simplifying the conditions of the experiment. The force between the interposed cylinder B and the disc *m*, being, within certain limits, as the squares of the distances *m n* inversely, and as the square of the number of charges directly; I was consequently enabled to select such distances and forces as were best adapted to the particular case. The influence of small variations in the distance *m n* on the inductive susceptibility of B was thus avoided. Thus in the experiments given in Table IV. (B), the intermediate cylinder underwent nearly as much inductive change with the given quantity, when the disc *m* was ·4 distance, as at ·2, as appears by the reductions just given, and by the attractive forces being the same, or very nearly, the difference in the number of charges being small. Thus in the attractive forces (A) the charges were 9, 17 +, and 26; in the attractive forces (B) they were 10, 21, 32. As the numbers refer to small measures of the unit jar, the differences upon the whole quantity are not greater than might be expected. I have obtained other results, in which the numbers were nearly alike. The experiments here recorded, however, better represent the average results.

31. The influence of a free neutral disc thus opposed to the terminating plane surface of an insulated cylinder, B, fig. 14, being such as to increase the inductive capacity of the cylinder, we cannot always estimate by this method the corresponding inductive and attractive forces. Thus in the case of the three cylinders, Exp. 25, Table III., in which the attractive force was as the altitude, we could not by this

method estimate the inductive change, since the influence of the suspended free disc would be such as to give each the same inductive susceptibility at the lower attracting surface. It is, however, to be observed, that the attractive forces are, there also, the same as in the case of taking the cylinders in a free state. In this case, therefore, the induction must be measured by a process similar to that resorted to (10.) Exp. 4.

32. Considering these results of consequence to a true theory of electricity, I thought it desirable to institute other methods of experiment, so as to expose more completely the operation of the inductive process, and at the same time verify the preceding deductions. With this view I resorted to the method represented in figs. 16 and 17, in which S T represents a cylindrical wood column attached to the foot-piece W, or substituted for the part P *g f*, fig. 1. This column carries the brass sliders S T. The slider S sustains the light tubular brass rod S V' V, and smaller sliders V V'; these support, by the glass rods V' A, V B, the conducting cylinders A, B. In like manner the sliding piece T sustains the thin slice *f*, fig. 16, forming a false upper end to B; or otherwise, if this slice be placed at the lower extremity of B, as in fig. 17, it is supported by the slider V, whilst T carries the body B. Now it is easy by this arrangement to remove, by the slider S, the bodies A and B, fig. 16, simultaneously, and without interfering with distance *a b*, so as to leave the thin slice *f* in operation on the electrometer disc *m*. Hence, if we suppose in this case, that A is charged with a given quantity, and B neutral, we may measure by the electrometer disc *m* the result of the direct inductive force upon *f*, apart from the bodies A and B, and this may be done without the result being influenced by the presence of the electrometer disc *m*, which may be temporarily turned aside during the previous process. We may also in fig. 17, supposing B charged and A neutral, examine the reflected induction by withdrawing A and the false end *b* simultaneously, and finally estimating by the electrometer disc *m*, the proportion of the whole charge abstracted by the influence of A at different distances. We may, in fact, obtain any required complicated mechanical arrangement peculiar to this kind of research, and arrive at a very complete experimental analysis of the reciprocal inductive action between the two opposed bodies, under a variety of new conditions*.

Exp. 27. The body A, fig. 16, being charged with a given quantity and placed within ·2 distance of B, the force upon the electrometer disc amounted to 20°; the distance *m f*, of the latter being ·5 of an inch. The cylinders A and B were now withdrawn simultaneously, leaving the false end *f* in place. The force amounted now to 8° only. This process was repeated with the distance $a\ b = {\cdot}4$, in which case the remaining force was 4°, or one half the former. The induction, therefore, as expressed in degrees of the electrometer, was as the distance *a b* inversely, and, consequently, the respective quantities of electricity left on the false end *f* as $\sqrt{1} : \sqrt{2}$ (4.). The quantity of electricity displaced, therefore, varied as the square root of the distance

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* To prevent the exposure of any additional surface on the removal of the false end, the cylinders were hollowed out for about an inch within the extremity, upon which the false end rested.

a b, being the law arrived at in the preceding experiments (29.). I extended this to distances ·6 and ·8, and still found the force in degrees as these distances inversely.

33. Exp. 28. The object of this experiment was to discover the resulting negative state induced in B under the influence of A, by touching it with a conducting wire and then removing A. With this view A was charged as before, and B rendered negative at the distance ·2; after this, A was withdrawn and discharged, and the negative force observed, which amounted to 20°, being the same as the previous induced positive state. This being repeated at distances ·4 and ·8, the respective negative forces were 10° and 7°, being in an inverse ratio to the distances, as before, and identical in this case with the previously induced positive forces, these last being observed upon the whole mass B whilst under the influence of A.

I verified this experiment by charging A so as to induce 5° and 20° of positive charge in B, that is to say, attractive forces in the ratio of 1 : 4, corresponding to quantities in the ratio of 1 : 2 (4.). On rendering B negative at the same constant distance *a b*, the forces were still 5° and 20° of negative charge. I found also on further repetition, that the same result ensued in multiplying the number of measures simply. Thus the negative force induced in B by three measures, being 10° at ·4 distance, six measures induced 40°, or very nearly; when therefore the charge in A is doubled, the induced negative state is also doubled in respect of quantity, since the corresponding degrees of the electrometer are quadrupled (4.). The following simple expressions may, therefore, be taken to represent this result: ind. $= \frac{Q^2}{D}$, if valued in degrees; or ind. $= \frac{Q}{\sqrt{D}}$, if valued in quantity.

34. Exp. 29. This experiment applies to the quantity of electricity influenced in B when charged by the opposed body A taken neutral and free, that is to the reflected induction (8.). The general arrangement is represented in fig. 17, in which the false end *f*, fig. 16, is placed at *b*, so as to detach it by the slider S, together with the neutral body A, by which we may discover how much of the whole quantity with which B is charged, considered as a unit of quantity, is determined as it were upon the near surface *b*, as also the respective quantities of electricity in operation between the opposed planes *a*, *b* at different distances. In conducting this experiment, the quantity with which B was charged $= m$, was first observed in degrees of the electrometer and taken as a unit of quantity, A being turned aside. Secondly, the false end *b* was removed, and the remaining quantity $= n$ also observed in degrees, so as to determine the decrease due to the removal of *b* alone $= t = m - n$. Thirdly, the false end *b* was replaced, the original charge made complete to $20° = m$, and the body A in a free state opposed to B at a given distance. Lastly, *b* and A were under this condition withdrawn together and the remaining quantity $= p$ observed in degrees, so as to obtain the comparative quantity actually existing in *b* whilst under the influence of $A = m - p$, as also the comparative quantity determined upon *b* by induction $= (m - p) - t$: putting $m - p = S$, we have the reflected induction in quantity

$= S - t$. Thus the whole quantity m being taken as unity or 1, the electrometer indicated 20° when the disc m was ·4 of an inch distant from the upper plane surface of B, the cylinder A being withdrawn. The false end b being now removed, the electrometer indicated 12°·5 : the quantity remaining therefore $= n$ was ·79 since (4.)

$$1 : n : : \sqrt{20} : \sqrt{12{\cdot}5}$$
$$: : 4{\cdot}472 : 3{\cdot}535$$

hence $n = {\cdot}79$. The decrease, therefore, due to b alone is in this case $= 1 - {\cdot}79 = {\cdot}21 = t$, hence about ⅕th of the whole of this particular charge was collected in the extremity b.

Now when b was withdrawn under the influence of A at distance ·2 then 3°·5 only remained: this corresponds to quantity ·42, nearly $= p$, since we have (4.)

$$1 : p : : \sqrt{20} : \sqrt{3^{\circ}{\cdot}5}$$
$$: : 4{\cdot}472 : 1{\cdot}872.$$

The decrease, therefore, due to b and A together is $1 - {\cdot}42 = {\cdot}58 = m - n = S$: hence about six-tenths of this particular charge was collected on the near surface b at distance ·2, and the quantity therefore determined there by the reflected induction is ${\cdot}58 - {\cdot}21 = {\cdot}37 = S - t$. These respective elements for different distances between the opposed surfaces of A and B determined as in the above example, are given in the next table.

Table V.

Original charge 20° = quantity 1 = m. Remaining deg. 12·5 = quantity ·79 = n. Quantity on b alone = ·21 = $m - n = t$.				
a.	*b.*	*c.*	*d.*	*e.*
Distance.	Final Degrees.	Quant. = p.	Quant. = S due to A + b.	Quant. S − t due to Ind. of A.
·2	3·5	·42	·58 +	·37 +
·3	5·5	·53	·47	·26 −
·4	7 +	·59	·41	·20
·5	8	·63	·37	·16
·6	9 +	·67	·33	·12 +

35. It may be inferred from this table, columns d and e, that the quantity of electricity (column d) accumulated in the near extremity b of the charged cylinder, fig. 17, was as the square root of the distance from the opposed surface of the neutral body, whilst the quantity displaced by induction of A (column e) is nearly in the inverse ratio of the distance. The numerical results are not everywhere rigidly exact, but they evidently point out these laws, and are in some cases extremely close. Thus in column e we have, taking the distances ·2 and ·6, which are as 1 : 3, the inverse quantities ·12 + and ·37 +, which are as 1 : 3; so also in column d we have corresponding to the same distances the quantities ·58 and ·33, which are to each other as the $\sqrt{3} : \overline{1}$.

36. The result shown in column *d* is strikingly in accordance with that arrived at by a former and distinct method of experiment (15.); by which it was found, that the reflected induction, if measured by the quantities of electricity the neutral body ceases to hold in equilibrio at different distances, is as the simple distance inversely. The result also of column *d* corresponds with the law just found (Exp. 27.) for the direct induction on the neutral body. The experiments, therefore, are clearly consistent one with the other.

When different charges were taken, and the distance *a b*, fig. 17, made constant, the numerical values varied with the charge, the forces being as the square of the charge expressed in degrees of the electrometer. This was at least observed for all the charges which could be fairly brought within the experimental range of the instrument.

37. The application of these results to the phenomenon of attraction by electrical agency, is not a little interesting; they in fact help us to a more complete perception of this wonderful operation. We may perceive, for example, that when a conducting substance charged with electricity attracts another conducting body in a free neutral state, the electrical distribution is so disturbed in each as to cause an accumulation in the opposed parts *a*, *b*, fig. 17, inversely proportional to the square roots of the distances. By the laws of electrical action, therefore, before explained, (4.) and (22.), Exp. 18, we have eventually the whole force, as shown by the electrometer, as the squares of the distances inversely. For let a unit of force at a unit of distance be given; suppose, for example, at the distance one inch; the force between *a*, *b*, fig. 17, was five degrees: let the distance be now taken = ·5, or one half the former, then the quantities of electricity in the opposed surfaces will be as 1 : 1·414, that is, as $\sqrt{1} : \sqrt{2}$ inversely (32.) (35.); but the force is as the square of the quantity. The force therefore with this change would be twice as great at the distance unity; but it varies also with the distance (Exp. 18.); hence at the distance ·5, or one half the former, it is again doubled; that is to say, the forces are as 1 : 4 when the distances are as 2 : 1, being a result of two simple laws taken conjointly, as already noticed (23.): similar reasoning applies to distances $\frac{1}{3}$, $\frac{1}{4}$ . . . $\frac{1}{n}$th, &c. When, as we have before explained (22.), the inductive changes are small, and admit of the quantity at the distance unity being taken as constant, then the force is as the simple distance inversely, depending solely on the closer approximation of the electrical particles (Exp. 18), or according to Faraday, on the diminution of the number of particles of the dielectric through which the force operates. Thus in the case of the attractive force between a charged and insulated neutral conductor, the induction may be very inconsiderable in respect of the whole charge. We observe in the free state, Table V. (*e*.), where the inductive force is the greatest possible, that at the distance ·4 not above one-fifth of the whole charge was determined towards the opposed surface; now these additions by the reflected induction of an insulated body may not greatly influence the result,

especially within certain limits; we may, for example, have so little as the $\frac{1}{300}$th and $\frac{1}{150}$th only of the whole charge disturbed at distances ·4 and ·2, which slight addition to the already existing accumulation in the opposed surface may not materially affect the electrometer: hence the force may vary nearly as the distance inversely. This, together with the circumstances above given (23.), may perhaps account for the difference in the law of the force, as regards the distance, between the insulated and neutral state. There are, however, probably, other conditions of induction already mentioned (26.) applicable to this case, and which the masterly investigations of Faraday bid fair to evolve: every one conversant with this interesting branch of science must necessarily allow, that never before has it been enriched by results so comprehensive and momentous as those contained in his several series of researches. It is also to be further considered, whether at small distances, although the force between two particles should be as $\frac{1}{D^2}$, still the force between the plates may be as $\frac{1}{D}$ simply, the whole attraction being found by a double integration, which sums all the forces, every particle of A being supposed to attract every particle of B.

38. The preceding facts lead us to refer every case of attraction in electricity, in a non-conducting or insulating medium, to the conditions of that peculiar combination of electrics and conductors, termed the Leyden jar, or coated pane, a combination consisting of an insulating body, interposed between two conducting surfaces. Now it is admitted that the charge which this combination can receive, is quite independent of the thickness of the opposed conductors, or of any hypothetical distribution upon them, or other bodies in connexion with them. Thus the charge which the electrical jar can receive under a given intensity, is as great when the coatings are mere films of metallic leaf, as when a solid mass of metal; the only condition essential to the perfect success of the experiment, is the free state of one of the coatings, and the complete insulation of the other, as also their close approximation. The action of the coatings in this case is reduced to one of these cases already given (16.), in which the distance between the bodies is constant, and the quantity of charge variable; the only difference being this, that in the case of the jar or coated pane, the intermediate insulator or dielectric is a solid, and the opposed conductors fixed, so that all motion by the resulting attraction is precluded, and all discharge between the conductors effectually prevented. The amount of charge which might be possibly collected on a small surface in this way, under a very dense atmosphere, is quite unknown. The charge might continue to accumulate until the resulting force between the opposed surfaces became so great, as to fracture the most impervious insulating substance placed between them.

39. The two following illustrations are conclusive of the general application of the laws above mentioned to the phenomena observable in accumulating electricity between two conducting surfaces under the conditions above mentioned.

1. Let A B, fig. 8, be two attracting plane areas, one of which is charged, and the

other, B, free; let the force between them with a unit of quantity = 1: suppose these areas to become now twice as great; then we have the charge distributed on twice the surface; and if we conceive it in each instance to be distributed equally, there would in the latter be then only one half the quantity in any given point, and hence, as found by experiment (16.) (18.), the indication of the force by the electrometer would become reduced to one fourth. In this state let the quantity be doubled, we have then by the same law (16.) the attractive force = 1, as before; that is to say, the charge which can be accumulated under a given attraction and distance between the plates, is as the opposed areas directly.

Now the indication of the charge by the electrometer, E, fig. 8, connected with the charged side, are, as we have seen (16.) (15.) (18.), proportional to the square of the quantity of electricity which the free surface ceases to hold in equilibrio. But the amount of charge and distance of the plates being constant, the quantity of electricity held in equilibrio will vary with the areas; and therefore if the area and quantity vary together, the electrometer will not change; hence it is, that a given number of degrees may, under this condition, correspond with any quantitative accumulation whatever.

If, then, we conceive in fig. 8. the two opposed plates to be the coatings of the intervening air, or any other dielectric, then, as just shown, the accumulation under a given intensity will be as the areas opposed.

2. Let the force between the plates A B, fig. 8, at a unit of distance *a b*, and with a unit of quantity = 1 as before, and suppose the distance *a b* to become now twice as great; then the force of attraction will be reduced to one-fourth, since it varies as $\frac{1}{D^2}$. Let the quantity under this condition be doubled, the attractive force will be = 1 as at first, since it is as the square of the quantity (4.); hence the accumulation between the opposed areas is under a constant attractive force directly as the distances between them.

Now the indications of intensity by an electrometer E, fig. 8, in connexion with the charged side, will be, as in the former instance, dependent on the reflected action of the free plate: this, taken in degrees of the electrometer, is as we have just seen (15.), as the squares of the distances inversely. If, therefore, with the quantity one, the distance *a b* be doubled, the intensity by the electrometer will be quadrupled: under this condition, let one half the quantity be again withdrawn, then the intensity by the electrometer will be the same as the first. If, therefore, we suppose, as in the preceding case, that the opposed plates are merely the coatings of the interposed dielectric, it follows that the charge under a given intensity will be as the distance between the plates inversely.

These deductions are in complete accordance with the many experiments made in this department of science by the learned Mr. Cavendish, who states in the 66th vol. of the Royal Society's Transactions, "that the quantity of electricity which coated glass can receive under the same degree of electrification is as the area of the coating directly, and as the thickness of the glass inversely."

It may be further remarked, that the force between two planes *m*, *f*, fig. 1, is not sensibly increased by increasing the area of one of the coatings only; is not influenced by the form or dimensions of the unopposed portions; and is the greatest possible when one of them is placed in a free state: circumstances which apply particularly to the case of charged glass. In considering the attractive force, therefore, between two conducting bodies of any form whatever, one of them being charged, the other free, it is only requisite to take into the calculation the opposed surfaces, and reason upon the inductive actions and distance according to the laws already given (27.) (32.) (34.).

40. Let for example the opposed bodies A, B be two spheres, as in fig. 18, or cones, or paraboloids, as in fig. 19, then the intervening dielectric will be of the form *a c e f b d a*, and the coatings *a d b c e f*, will be hemispherical or otherwise, according to the figures; and the unopposed portions, taking the bodies in a charged and free state, will not affect the result; they may be of any form, or have any connexion whatever with other bodies. We may always predict the attractive force, a unit of force at a unit of distance being given, on the supposition that it is as the opposed areas directly, and as the squares of the distances inversely; the electricity accumulated in the opposed points being as the square roots of the respective distances* (32.).

This general result is not vitiated by any oblique action which may appear to arise in consequence of change in the position of the opposed surfaces. Thus if the two opposed plane areas, *d*, *f*, fig. 20, be any how placed, provided they maintain their relative position with respect to each other, it is evident no difference can possibly arise; it would be in fact merely placing a coated pane in different angular positions. If we suppose one of the plates brought into the position *a b*, fig. 21, so as to be oblique to the other, still the same general principle applies: we may conceive the interval of air, *a n m b*, to be a solid dielectric of unequal thickness, the coatings of which are the opposed areas *a b*, *m n*; the attractive force, therefore, between the plates, *a b*, *a' b'*, would become diminished by the exposure of the unopposed portions, *n a'*, *m b'*, and by the general increase of distance. If the plates were so opposed as to cause a portion of one to project beyond the other, as in fig. 22, then the force would be reduced to the opposed portions, *a m*, *n d*, and would be diminished by the external unopposed parts *n c*, *m b*.

We suppose, however, in all these cases that the force is exerted between a neutral body in a free state, and a body charged with a quantity of electricity, considered indefinitely great with respect to the opposed surfaces; directly, however, we limit these conditions, either by insulating the neutral body or by narrowing the capacity of the charged body, then corresponding variations arise in the laws of the force, but which may be reduced to calculation on the general principles above stated (37.).

41. The influence of induction on the repulsive force evinced by similarly electrified substances is such as to merit very particular attention; I have shown in the

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* Transactions of the Royal Society for 1834, p. 240.

second series of these inquiries, that the operation of electrical repulsion is subject occasionally to considerable variation, the result being dependent on quantity, intensity, distance, and a variety of other contingent circumstances*. Without taking into account, therefore, the attractive forces generated between the discs, *m*, *f*, fig. 11, I could not, by the method of experiment before employed (24.), obtain any uniform result; the repulsive force appeared irregular, and in many cases capricious, appearing sometimes as great at one distance as at another: but in calculating first the force of the disc *f* when charged, on *m* taken as neutral, and then the force of *m* similarly charged, on *f* taken as neutral, the results, with the corrections thus deduced, were uniform, and according to a given law, as may be seen in the following experiment.

Exp. 30. Two discs, *m*, *f*, fig. 11, being opposed to each other, one of them *m*, was charged with a given quantity, and the other placed in connexion with a coated jar, charged with the same electricity; the charges in the latter were estimated by the unit measure. The elementary measurements were, first, the attraction of the suspended disc *m*, charged with a given quantity, on *f*, considered as neutral $= a$; secondly, the attraction of *f*, charged with a given number of measures, on *m*, taken as neutral $= p$; lastly, the repulsive force between the bodies with a given number of measures $=$ R. The attractive forces were neutralized by small weights placed on the scale pan *p*; the arm *g* of the balance, up to the instant of the repulsion, rested on a small support projecting from the brass work supporting the beam B. The repulsive force was estimated by weights either placed on the disc *m*, or otherwise removed from the scale pan, and by which the whole had been previously brought to balance.

The annexed table comprises the numerical results of a few experiments conducted in this way; the attractive force between *m* and *f*, taking *m* charged, varied as the square of the distance inversely, or very nearly, for the quantities employed. The force, taking *f* charged, was in general so small as to admit of being neglected (Exp. 22.).

TABLE VI.

Distance.	Attr. Force.	Rep. Force.	Quantity.
·4	5	9	38
·8	1+	4·5	36
1·2	0·3	3+	36
1·6	..	2+	37

It may be seen here that the repulsive force was in the simple inverse ratio of the distance, and that the number of measures for each experiment did not greatly differ.

A similar result was obtained by means of the electrometer, fig. 1: the attractive forces were here very easily determined, and the number of corresponding degrees noted, by employing charged glass, as in Experiment 22. The repulsive forces alone were obtained; they were, however, still in the simple inverse ratio of the distances.

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* Transactions of the Royal Society for 1836.

42. These phenomena may not be unimportant to further advances in this department of science; they rather lead us to consider electrical attraction as essentially differing in its nature from forces emanating from a centre, and go far to assist us in elucidating many of its operations, hitherto considered of a complicated character. Thus the attractive force between spheres and bodies of other forms has given rise to a mathematical analysis of some difficulty*, which although displaying the highest order of talent is certainly not indispensable. We may on very simple principles determine, as already shown (40.), the laws of the force between bodies of any form, whether insulated or free, whether charged positively or negatively, or whether electrics or conductors.

43. The following are a few simple expressions which may be taken to represent some of the elementary laws of electrical induction and attraction, in which Q = quantity of charge, T the direct induction, q the quantity of electricity displaced, t its intensity, T' the reflected induction, q' the disturbed quantity, t' its intensity, q'' the total quantity in the opposed charged surface, A the surface, D the distance between the opposed points, F = force of attraction.

We have then for the direct induction

$$T = q = \frac{Q}{\sqrt{D}}, \qquad t = \frac{Q^2}{D}.$$

For the reflected induction T' we have

$$T' = q' = \frac{Q}{D}, \qquad t' = \frac{Q^2}{D^2}, \qquad q'' = \frac{Q}{\sqrt{D}}.$$

We have for the attractive force between a charged and neutral free conductor

$$F = \frac{Q^2}{D^2}, \qquad F = \frac{T}{A^2}.$$

For the force between an unchangeable positive and negative surface we have

$$F = \frac{Q^2}{D}.$$

44. In these inquiries I have not resorted to the view of electrical action I was led to entertain in the first series of these papers, in which a portion only of the whole charge is supposed to be appreciable by the electrometer, being unwilling to embarrass the inquiry with theoretical speculations not essential to a full development of the experimental facts. I may, however, still observe, that the present state of this department of science does not warrant any very perfect confidence in the common mechanical explanations of the mode of operation of electrical forces generally, and which after all seems to be of a cumbrous and difficult character. If we suppose a particle of the electricity = a on a charged body A, fig. 8, to attract every par-

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* Supplement Encyclop. Britt., Article ELECTRICITY.

ticle of the opposite electricity = *b* on the opposed body B, instead of supposing the force to be confined to the near particle immediately opposed to it, we cannot take all the forces as equal, and the whole force to be as *a b*; it must still be only as some function of it, and we have to sum the forces under this condition. But in this case even, all the oblique actions may at last be indefinitely small in respect of the force exerted upon the opposite and nearest particle, which would still admit of the whole action being reduced to a system of parallel forces, such as represented in fig. 18, more especially when we take into consideration the fact that the force between two particles, *a*, *b*, fig. 23, is greatly diminished, and may become very small by placing a similar third particle, *d*, nearer either of them, supposing it charged with the opposite electricity; and this result will be again augmented by placing another particle, *e*, nearer the other in a similar way. In this case the action between the original particles *a*, *b* almost vanishes.

45. In concluding this communication it may not be improper to state, that the experiments were conducted in a good insulating atmosphere, generally in a room dried by an air stove: the late contrivance of Dr. ARNOTT is quite invaluable to the practical electrician for this purpose. The manipulations requiring especial care are, 1. Measure of quantity; 2. of distance; 3. Adjustment of the electrometer, especially in such experiments as No. 26; 4. Perfection of the insulators. The circumstances liable to interfere with a rigid numerical result are, slight changes in the position of the bodies under experiment,—the bodies should be firmly steadied; inaccuracy in the value of the unit measures, either by less perfect insulation of the air, or by other causes; a want of free connexion of the external coating of the jar, P, fig. 16, with the ground; small residuary charges in the discs of the electrometer; these should be always discharged by a bent wire at each experiment. A series of delicate manipulations of this kind, although apparently difficult, may yet by a little habit and attention be completely managed.

Plymouth,
April 10, 1839.

XV. *On the Conditions of Equilibrium of an Incompressible Fluid, the Particles of which are acted upon by Accelerating Forces.* By JAMES IVORY, *K.H. M.A. F.R.S. L. & E., Instit. Reg. Sc. Paris, Corresp. et Reg. Sc. Gottin. Corresp.*

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EXPERIENCE shows that physical problems of difficulty are never solved in a satisfactory manner but after reiterated attempts. The examples that might be adduced in support of this remark, are too obvious and numerous to need particular mention. A remarkable instance is the problem of which it is proposed to treat in this paper, namely, that relating to the figure of equilibrium of a mass of fluid, the particles of which are subjected to the action of accelerating forces. This problem, suggested by the inquiry into the figure of the planets, was first treated of by NEWTON and HUYGHENS; it then passed into the hands of MACLAURIN, CLAIRAUT, and D'ALEMBERT; and it finally occupied the attention of EULER, LAGRANGE, and LAPLACE, by whose researches it is declared on high authority* that the solution is completed, leaving no difficulties, except of a mathematical kind, in applying it to any case that may be proposed. The theory thus finally settled is imposing by its great generality and apparent simplicity; it succeeds in solving a certain class of problems, although not on sound principles; but in other instances no degree of mathematical skill has been able to obtain satisfactory results. A candid inquirer who will endeavour to form just notions of the conditions required for the equilibrium of a fluid, will not fail to have his attention arrested by much that is inconsistent and obscure in the usual manner in which this subject is treated. This seems to imply some imperfection in the grounds of the theory; and the best way of removing all difficulties is to mount up to the origin of the inquiry, and to trace it with careful examination through all its successive steps. In this manner we may detect what is defective or erroneous; and having arrived at physical conditions not liable to objection or uncertainty, the theory may be placed on a firm foundation.

It will not be necessary to say a word on the importance of a theory which has occupied the attention of so many eminent geometers, and which is the subject of no small part of what has been written on the system of the universe. As it treats of the figure of a fluid, it seems to suppose that the earth and planets were originally in a state of fluidity, either by the solution of their solid parts in a liquid, or by the effect of heat. Now as we have no knowledge of the primitive condition of the bodies of our system, it may be objected that the problem, whatever ingenuity may be required to overcome its difficulties, is merely speculative and hypothetical. But the matter may be viewed in a different light. No small progress has already been made in the investigation of the figure of the earth; and our knowledge in this respect may be made more perfect by assiduous observation and discussion: we are also acquainted with all the forces, whether attractive or centrifugal, that urge every particle of the matter of which our globe is composed; and hence, reversing the usual question, the inquiry may be, whether a change in the actual figure of the earth would necessarily take place if the bonds that hold together its solid parts were loosened, and a state of fluidity induced upon the whole or any portion. A speculation of this kind at any time, and in every revolution imposed by fashion on scientific research, may be deemed not altogether uninteresting, and may be useful in studying the changes that take place on the surface of our globe.

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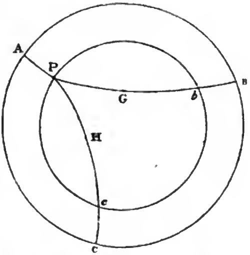
* M. POISSON.

1. It is obvious that a homogeneous body of fluid, the particles of which are not agitated by extraneous forces, but are left freely to their mutual action on one another, will ultimately assume the figure of a perfect sphere. In treating of the figure of the earth, NEWTON supposes that this sphere is made to revolve about one of its diameters; in consequence of which the centrifugal force will cause the fluid to recede from the axis of rotation, and to subside in the direction parallel to that axis. He makes no inquiry into the nature of this new figure, but immediately concludes, without assigning a reason, that it is an oblate elliptical spheroid turning about the less axis.

It would be in vain to inquire on what grounds NEWTON inferred that the revolving sphere is changed into an exact oblate spheroid. The flattening at the poles suggests a resemblance of the two figures; and we may add that, by making the two axes of the spheroid more and more unequal, it will pass through all degrees of oblateness, and may be supposed, in some one of its forms, to coincide with the flattened sphere, if not exactly, at least with a sufficient approximation.

Having guessed the form of equilibrium, the main difficulties of the research were overcome; for it is much easier to investigate the properties of a known figure, than to determine the form itself which is required for an equilibrium. He begins with laying down this principle, that in a fluid spheroid in equilibrium, the weights, or efforts, of all the small rectilineal columns extending from the centre to the surface, must balance one another round the centre. Assuming a spheroid, of which the axes are very nearly equal, namely, 101 and 100, he computes, by means of his assumed principle, and some propositions in his immortal work, the weights at the centre, caused by the attraction of the matter of the spheroid, of two columns, one drawn in the plane of the equator, and the other to the pole. The spheroid must next be supposed to revolve about the polar axis, so that the centrifugal force, by diminishing the weight of the equatorial column, may equalise the weights of both columns at the centre, as an equilibrium requires; and when this condition is fulfilled, it is found that the oblateness of the spheroid, or the difference of the two semi-axes in parts of the equatorial radius, is to the centrifugal force at the equator in parts of the gravity, as 5 to 4. Now this proportion of 5 to 4 is common to all spheroids of which the axes are nearly equal; and as the centrifugal force at the earth's equator is $\frac{1}{289}$ of gravity, the oblateness of the terrestrial spheroid will be $\frac{5}{4} \times \frac{1}{289} = \frac{1}{230}$, making the proportion of the polar axis to the diameter of the equator as 229 to 230. Such would be the true figure of the earth if its matter were homogeneous, and if Newton's reasoning were liable to no objections. All that the present purpose requires to be noticed in his very able investigation, is the principle of the equiponderance of the central columns which he introduced.

Newton had occasion to consider no figure but the elliptical spheroid, in which, from its symmetry, the equiponderance of the central columns is self-evident, every column being counteracted by an equal and similar column diametrically opposite. In a body of fluid at liberty and in equilibrium by the action of accelerating forces, is there always a central point round which the efforts of the whole mass balance one another? It is obvious that all canals extending from a particle to the surface of the fluid, will impel the particle with equal intensity; for otherwise the particle would not be at rest. What is it, then, that distinguishes the equal pressures upon a particle in any situation, from the like pressures on the central point, if there be such a point? In a research in which there have occurred so many inadvertencies arising from hypothetical admissions, it is necessary to inquire in what manner the pressures are distributed among the particles.



Let A B C represent a mass of fluid in equilibrium; P any particle; P A, P B, P C, small canals diverging to the surface of the fluid: as P is at rest, the efforts of all the canals will balance one another. In passing along any canal A P B ending both ways in the surface, the pressure, which is zero at A, will first increase to a certain point G, after which it will decrease to zero at B. Because the pressure from A to G is equal and contrary to the pressure from B to G, there is always a part B *b* at one end, which presses inward with an intensity equal to the like effort of the part A P at the other end. Thus, of the three parts of the canal, the forces which urge the fluid in the two extreme parts transmit equal and contrary pressures to P; but the forces acting on the fluid in the middle part P *b*, destroy one another's effects, and cause no pressure on P. The same thing is true of any other canal A P C; the effort of the part A P on one side of the point of maximum-pressure H being balanced by the contrary effort of the part C *c* on the other side of that point. Now if we suppose a curve surface to be drawn through P, and through all the points *b*, *c*, &c., it follows from what has been proved, that the pressures which impel P with equal intensity in all directions, are caused solely by the forces which urge the portion of fluid on the outside of that surface; for any canal being drawn between P and the upper surface, it is only the part of such canal between the two surfaces that transmits an effective pressure to P.

Such interior surfaces as P *b c* are called level surfaces. Every level surface is pressed at all its points with the same intensity by the exterior fluid; for the forces acting on the particles contained in any canal between a level surface and the upper surface, produce the same effort directed inward.

If we conceive that the interior surface P *b c* gradually lessens in its dimensions by the increasing depth, it will finally contract into a point; which point, or centre, is distinguished from every other point P, by this, that the pressures which impel it equally in all directions are produced by the forces which urge all the particles of the body of fluid.

The central point is distinguished by another property peculiar to it: for the pressure being a maximum, the partial differentials of the pressure, or the forces, will be zero: so that if a particle in the centre be removed a little in any direction, there will be no sensible change of the pressure upon it. Thus the centre is in stable equilibrium with respect to the action of the whole mass of fluid. Although any other particle, as P, is pressed equally on all sides, yet, as the forces in action are not zero, the change of pressure which it undergoes when moved a little from its place, will be different according to the direction of its motion; unless it be moved so as to continue upon the level surface P *b c*, in which case the pressure, being still produced by the action of the same portion of the whole mass, will not vary.

What has been said establishes NEWTON's equiponderance of the central columns as a general principle of equilibrium that holds in every case of a mass of fluid at liberty, the particles of which are urged by accelerating forces.

Enough has also been said to demonstrate the insufficiency of the principle of equality of pressure for determining the figure of equilibrium of a fluid. For it has been shown that the equal pressures which a particle sustains have no other effect than to make it immoveable by the action of the portion of fluid on the outside of the level surface that passes through it: but from this it does not follow in all cases that the particle is reduced to a state of rest relatively to the whole mass. The same theory tacitly assumes that every body of fluid contained within a level surface will maintain its form and position, merely by the equal pressures of the exterior fluid; not adverting to the necessity of taking into account all the forces of whatever description that act on the particles. In the foregoing investigation it is clearly proved on the supposition of an equilibrium, that the forces in action must be without effect to cause pressure either way in any canal, as P *b* or P *c*, within a level surface.

2. Some years before the publication of the Principia, it had been ascertained by observation that the same mass of matter has not the same weight at all the points of the earth's surface. A pendulum clock regulated by mean time at Paris was found by M. Richer to lose two minutes a day at Cayenne, within 5° of the equator. Now the length of a pendulum that oscillates in a given time is an exact measure of gravity; and the fact observed by M. Richer proved that, if a heavy body were carried from Paris to Cayenne, it would lose some part of its weight. On learning this fact, Huyghens conjectured that it was caused by the centrifugal force arising from the daily revolution of the earth; the intensity of this force varying in different latitudes at different distances from the axis of rotation. Combining this observed variation of gravity with a principle, which is indisputably true, namely, that a plumb-line freely suspended is perpendicular to the surface of standing water, or to the surface of the earth supposed entirely fluid, he drew an argument, that the earth is not exactly spherical. Were the earth a perfect sphere, the attraction of its mass would be perpendicular to the surface at every point: the centrifugal force, directed at right angles from the axis of rotation, is oblique to the surface: wherefore gravity, being the resultant of both forces, and consequently not coinciding in its direction with either, would not be perpendicular to the surface, which is contrary to the admitted principle. It will readily appear that the resultant of the two forces, or the true direction of the plumb-line, would always make a small angle with the radius of the sphere on the side towards the equator: so that a horizontal plane perpendicular to this direction, would necessarily fall within the sphere towards the pole: which is a direct proof that the surface of the earth, formed by all such horizontal planes, is depressed at the poles. These speculations of Huyghens are contained in his dissertation, *De causa gravitatis*: in an addition to that dissertation which was published after the author's death, he proceeds to investigate the oblateness of the earth, or the difference between the equatorial radius and the polar semiaxes, caused by the centrifugal force. By this time the Principia was published: but the doctrine of the mutual gravitation of all matter, was at first generally objected to, and forced its way very slowly to universal approbation. Huyghens, rejecting the principle that every particle of matter gravitates to every other, substituted in its place a tendency of all the parts of the earth supposed in a fluid state, to the common centre of the mass, the central force acting with the same intensity at all distances. Following the method devised by Newton for the solution of the same problem, the simple law of gravity adopted by Huyghens leads to an easy solution: because a narrow rectilineal canal of any length drawn from the centre, will have a weight proportional to the quantity of fluid it contains. If a fluid spheroid revolve about the polar axis with a circular velocity capable of impressing on the particles a centrifugal force the intensity of which is to that of gravity as n to 1, it is easy to prove that the centrifugal force will diminish the weight of a canal in the plane of the equator, and equal to the radius of that circle, or to 1, by the quantity $\frac{n}{2} \times 1$: so that the whole weight

of the canal from being equal to 1 will be reduced to $1 - \frac{n}{2}$; which will therefore be the length of a canal reaching from the centre to the pole equiponderant to the equatorial canal. Applying this result to the earth, we have $n = \frac{1}{289}$, and the proportion of the radius of the equator to the polar semi-axis equal to 578 to 577, the oblateness being much less than in the Newtonian Theory.

Huyghens next attempts to investigate, what form the perpendicularity of gravity to the earth's surface requires the terrestrial meridians to have. But in this part of his researches no result is obtained which it would be useful to notice. He finds indeed a curve which, in his law of gravity, answers the mathematical conditions: but this curve is a paraboloid consisting of two infinite branches that diverge continually from one another; a form irreconcilable with the continuous surface of the earth, every meridian of which is an oval curve returning into itself. It appears from what has been said that the contribution of Huyghens to the theory of the equilibrium of fluids, must be limited to the perpendicularity of the resultant of the forces to the surface, which principle he was the first to suggest.

3. We have now two properties that must be verified in every mass of fluid at liberty and in equilibrium by forces acting on its particles, namely, the equiponderance of the central columns of which Newton is the author, and the perpendicularity to the surface of the resultant of all the forces urging a particle, which was proposed by Huyghens. But it is one thing to detect particular properties of an equilibrium, however general in their application, and another thing to fix with precision the conditions necessary for inducing that state on a mass of fluid acted upon by given forces. In solving problems, geometers sometimes made use of one principle, and sometimes of the other. It was soon found that a figure obtained by means of one principle, did not in all cases verify the other; and even that the concurrence of both in the same mass of fluid was not sufficient in some instances to ensure an equilibrium. From all this it could only be inferred that the problem was still involved in obscurity, and required to be further discussed.

4. In the Principia Newton has completely determined the attraction of spheres. He has also given methods for determining the attraction of other bodies; which methods, although sufficient for obtaining numerical results, fail for the most part in ascertaining the law according to which the attractive force of the mass will vary when the attracted point changes its place. Maclaurin, by a happy application of the ancient geometry, determined this law in elliptical spheroids of revolution, for all particles within the solid or in its surface. He found that the mass of the spheroid attracted a particle so situated, in directions perpendicular to the plane of the equator and to the axis of rotation, with forces respectively proportional to the distances from the plane and from the axis*. Now the centrifugal force is directly proportional to

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* Although Maclaurin's demonstration rests on this property, yet this property itself is essentially dependent on another property, which the author has demonstrated in his Fluxions, § 630.

the distance from the axis of rotation: and thus was known every force tending to move any particle of an elliptical spheroid revolving about its axis in a given time. The difficulty of estimating the forces and pressures in different parts of the spheroid, obliged NEWTON to confine his attention to the central columns. The discovery of MACLAURIN removed this difficulty, and enabled him to ascertain whether the spheroid fulfilled any proposed property of equilibrium, or not. He first determines the relative dimensions of the spheroid, which are necessary for making the resultant of the attractive and centrifugal forces perpendicular to the surface at every point, as is required by the principle of HUYGHENS; and he demonstrates that the same figure verifies NEWTON's principle of the equiponderance of the central columns. Taking now any particle, or small portion of the fluid, and conceiving that it is pressed on every side by rectilineal canals standing upon it and terminating in the surface, he showed that the pressures of these canals impel the particle equally in all directions. These several points are demonstrated with the utmost elegance, and with all the rigour of EUCLID or ARCHIMEDES.

Such is the celebrated demonstration of MACLAURIN, which adds the property of every particle being pressed equally in all directions, to what NEWTON and HUYGHENS had before shown to be necessary for an equilibrium. But on reflection it is not quite clear how this new property is to be understood or what use is to be made of it. There is no doubt that most authors infer, from the equal pressures which a particle sustains on every side, that it it is necessarily brought to a state of rest within the spheroid; and hence the equality of pressure has been erected into a general principle, on which is founded the usual theory of equilibrium. But what MACLAURIN does really demonstrate amounts to this*, that a particle placed on an elliptical surface similar and concentric to the surface of the spheroid, is impelled by any rectilineal canal standing upon it and terminating in the surface of the spheroid, with a pressure equal to the effort of a given canal having for its length the difference of the polar semi-axes of the two similar surfaces. Now the proper inference certainly is, that the particles at every point of the interior surface press upon one another, and upon the surface in which they are placed, with the same intensity. To say that a particle is pressed equally in all directions, is tantamount to saying that it is placed on a level surface; every particle on such a surface being urged equally on all sides by the exterior fluid, either by direct action, or by the efforts transmitted through the fluid contained within the surface. But it cannot be reasonably inferred, as is done in the theory of equilibrium founded on equality of pressure, that a particle is reduced to a state of rest relatively to the whole of a body of fluid, merely because it is pressed equally in all directions by a portion only of the mass.

In the treatise on Fluxions published in 1742 MACLAURIN does not affirm explicitly that a particle is at rest within the spheroid, because it is pressed equally on all sides; although it is undoubtedly implied that this is true: and he concludes his investigation* with saying, that the surfaces similar and concentric to the surface of the spheroid, are the true level surfaces at all depths; which is alone sufficient for the equilibrium, and is indeed the simple and direct and the only exact ground of the demonstration, agreeing perfectly with what has been advanced. In the Dissertation presented by the author to the Academy of Sciences in 1740, the matter is differently stated. Having proved, in his first proposition or *Theorema Fundamentale*, that any particle is impelled equally in all directions by a certain force depending only on the position of the particle, or rather on the surface passing through the particle similar and concentric to the surface of the spheroid, he adds, "*quæ (particula) cum æqualiter urgeatur, fluidum est ubique in equilibrio.*" Here it is unequivocally asserted that the equal pressures which a particle sustains, reduce it to a state of rest within the spheroid. This would be correct if it were proved that the equal pressures are produced by the action of the whole mass of the spheroid. That a particle is pressed equally by the surrounding fluid, and that it is at rest within the spheroid, are two distinct propositions, of which the second is not necessarily a consequence of the first: for the equal pressures may be caused by the action of only a part of the fluid; whereas the effect of the forces that act upon all the particles must be taken into account, in order to prove that a particle is at rest relatively to the whole mass. What MACLAURIN has accurately proved of one particle, holds equally of all the particles situated in any surface similar and concentric to the surface of the spheroid, the pressure on all such particles being the same; and the proper inference to be drawn is what the author has stated in his Fluxions, namely, that all such interior surfaces are the true level surfaces at all depths.

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* Fluxions, § 639.

5. In proving that the pressure upon any interior particle of the spheroid is equal in all directions, MACLAURIN used rectilineal canals; but it is evident that the effect must be the same, whether the canals be rectilineal, or have curvilineal figures varied in any manner. From observing that, in a fluid at rest, the pressure of a canal will be the same when its extreme points are the same, however its form may be varied, CLAIRAUT deduced a relation between the figure of a fluid in equilibrium and the mathematical expression of the pressure, or of the forces which produce the pressure. This consideration greatly simplified and improved the theory of equilibrium, as it made it unnecessary to seek after such artifices of investigation as MACLAURIN was obliged to have recourse to. This property is enunciated in the following theorem.

Theorem.—In a fluid at rest by the action of accelerating forces on its particles, the mathematical expression of the pressure at any point of the mass can be no other than a function of the three co-ordinates of the point, these co-ordinates being considered as independent and unrelated quantities.

Demonstration.—Let a communication be opened between any two points of the mass of fluid by means of a canal of any figure; because the fluid is supposed to be at rest, the pressure of the fluid in the whole length of the canal will be equal to the difference of the pressures in the body of fluid at the two orifices, which pressure will therefore remain the same, however the figure of the canal be varied. It is easy to ascertain that this property will be verified when the pressure at any point of the fluid is represented by a function of the co-ordinates, such as is described in the theorem. Thus the co-ordinates of the two orifices of the canal being represented by a, b, c and a', b', c'; and the pressures at the same points by $\varphi(a, b, c)$ and $\varphi(a', b', c')$; through whatever gradations the figure of a canal requires that the independent variables a, b, c be made to pass so as finally to become equal to a', b', c', the function $\varphi(a, b, c)$ will always be changed into $\varphi(a', b', c')$.

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* Fluxions, § 640.

But the theorem may be regularly demonstrated in the manner following. Let the variables x, y, z represent the co-ordinates, and $\varphi(x, y, z)$ or φ the pressure at a point of the fluid: if the co-ordinates vary in the curve of the canal, the sum of the differentials in the whole length of the canal will be

$$\int\left(\frac{d\phi}{dx}dx + \frac{d\phi}{dy}dy + \frac{d\phi}{dz}dz\right);$$

and the sum of the variations of all these differentials in another canal very near the first and between the same extreme points, will be

$$\int\delta.\left(\frac{d\phi}{dx}dx + \frac{d\phi}{dy}dy + \frac{d\phi}{dz}dz\right);$$

and the condition that the pressure caused by the efforts of the fluid in one canal is not different from the like pressure in the other canal, is thus expressed:

$$0 = \int\delta.\left(\frac{d\phi}{dx}dx + \frac{d\phi}{dy}dy + \frac{d\phi}{dz}dz\right).$$

When this last expression is integrated by parts, we obtain

$$\begin{aligned}0 = {} & \frac{d\phi}{dx}\delta x + \int dx\,\delta.\frac{d\phi}{dx} - \int\delta x\,d.\frac{d\phi}{dx} \\ & + \frac{d\phi}{dy}\delta y + \int dy\,\delta.\frac{d\phi}{dy} - \int\delta y\,d.\frac{d\phi}{dy} \\ & + \frac{d\phi}{dz}\delta z + \int dz\,\delta.\frac{d\phi}{dz} - \int\delta z\,d.\frac{d\phi}{dz}.\end{aligned}$$

Now the terms without the sign of integration in this expression are zero when the integrals are extended to the whole lengths of the canals; for the extreme points being fixed, the variations of the co-ordinates at these points are zero. We thus obtain by an easy reduction,

$$\begin{aligned}0 = {} & \int(dx\,\delta y - \delta x\,dy)\cdot\frac{dd\phi}{dx\,dy} + \int(dx\,\delta z - \delta x\,dz)\cdot\frac{dd\phi}{dx\,dz} \\ & + \int(dy\,\delta x - \delta y\,dx)\cdot\frac{dd\phi}{dy\,dx} + \int(dy\,\delta z - \delta y\,dz)\cdot\frac{dd\phi}{dy\,dz}\end{aligned}$$

$$+\int(d z\,\delta x-\delta z\,d x)\cdot\frac{d d \phi}{d z\,d x}+\int(d z\,\delta y-\delta z\,d y)\cdot\frac{d d \phi}{d z\,d y}:$$

which equation will be verified only by means of these formulas, viz.

$$\frac{d d \phi}{d x\,d y}=\frac{d d \phi}{d y\,d x},\quad \frac{d d \phi}{d x\,d z}=\frac{d d \phi}{d z\,d x},\quad \frac{d d \phi}{d y\,d z}=\frac{d d \phi}{d z\,d y}.$$

Now these equations express the well-known conditions that the variables of the function $\phi\,(x, y, z)$ are independent quantities; which sort of function is therefore alone fit to represent with consistency the pressure of a fluid at rest.

The nature of the pressure in a fluid at rest being ascertained, the expressions of the forces which produce the pressure follow as a corollary; for these expressions are necessarily no other than the partial differentials of the pressure. Supposing always, for the sake of simplicity, that the density is constant and equal to unit, let X, Y, Z represent the forces which, acting in the respective directions of the co-ordinates, produce the pressure $\phi\,(x, y, z)$ or ϕ; we shall have

$$\mathrm{X}=\frac{d \phi}{d x},\quad \mathrm{Y}=\frac{d \phi}{d y},\quad \mathrm{Z}=\frac{d \phi}{d z}.$$

From the foregoing equations between the differentials of ϕ, the following formulas are easily deduced, viz.

$$\frac{d \mathrm{X}}{d y}=\frac{d \mathrm{Y}}{d x},\quad \frac{d \mathrm{X}}{d z}=\frac{d \mathrm{Z}}{d x},\quad \frac{d \mathrm{Y}}{d z}=\frac{d \mathrm{Z}}{d y}.$$

These equations are the known conditions for the integrability of the differential

$$\mathrm{X}\,d x+\mathrm{Y}\,d y+\mathrm{Z}\,d z:$$

and they must be verified in every problem by the proposed forces, otherwise the equilibrium of the fluid will be impossible.

If p represent the pressure estimated on a given surface at the point $(x\,y\,z)$ of the fluid, we shall have,

$$p=\phi\,(x, y, z), \quad \ldots\ldots\ldots\ldots \quad \text{(A.)}$$
$$p=\int(\mathrm{X}\,d x+\mathrm{Y}\,d y+\mathrm{Z}\,d z),$$

the integral extending from the point to the surface of the mass. We next obtain these differential equations,

$$d p=\frac{d \phi}{d x} d x+\frac{d \phi}{d y} d y+\frac{d \phi}{d z} d z,$$
$$d p=\mathrm{X}\,d x+\mathrm{Y}\,d y+\mathrm{Z}\,d z:$$

and if we suppose $d\,p=0$, or that the co-ordinates vary in a surface at every point of which the pressure is the same, that is, in a level surface, the surface of the mass being included, there will result

$$0=\frac{d \phi}{d x} d x+\frac{d \phi}{d y} d y+\frac{d \phi}{d z} d z,$$
$$0=\mathrm{X}\,d x+\mathrm{Y}\,d y+\mathrm{Z}\,d z.$$

Now these last equations prove that the forces urging a particle in any level surface have no effect to move the particle in any direction upon a plane touching the surface; from which it follows that the resultant of such forces is perpendicular to the surface in which they act. It is also obvious that the resultant is always directed towards a level surface and towards the surface of the mass.

The equations that have been investigated are common to every mass of fluid at liberty and in equilibrium by the action of accelerating forces on its particles. The same equations contain all that is taught in the usual manner of treating this subject. If we suppose that the pressure p remains constant, while the co-ordinates vary, the equation (A.) will determine a level surface. If the point at which the pressure is p remains fixed, while the co-ordinates vary in any canal terminating in the surface of the fluid, the same equation (A.) proves that all the canals will exert the same pressure p upon the point. Every point in a level surface is impelled in all directions with the same intensity of pressure; and nothing is gained, but the hazard of misconception is incurred, by applying equality of pressure to isolated points.

MACLAURIN's demonstration will always be admired; but that geometer was unfortunate in considering the pressure upon an isolated point. If he had observed, what follows from his reasoning, that every particle in a surface similar and concentric to the surface of the spheroid is pressed equally on all sides, he would have been led to the property of the level surfaces of which he has ultimately made use in his Fluxions, and which is the true principle of the equilibrium of a fluid, namely, that the level surfaces at all depths must have determinate figures.

Although the equation (A.) is common to every fluid in equilibrium, it does not follow that every problem can be solved by one equation. The level surfaces depend upon the proposed forces; and they require for the determination of their figure as many independent equations as these given forces that derive their origin from independent sources.

The not observing that in every canal terminating both ways in the surface of the fluid there are always two points pressed inwards in contrary directions with the same intensity, and consequently an intermediate part which presses neither way, has occasioned the misconception, from which much confusion has arisen, that the equal pressures of the surrounding fluid upon a particle necessarily reduce it to a state of rest within a body of fluid in equilibrium.

6. The following Problems are added for elucidating the principles that have been investigated.

PROBLEM I.

To determine the figure of equilibrium of an incompressible fluid when the forces are such functions of the co-ordinates as are susceptible of only one value for any proposed values of the co-ordinates.

Solution.—Let x, y, z denote the co-ordinates of a particle of the fluid, p the pressure, and X, Y, Z the forces acting in the respective directions of x, y, z; from the equation (A.) we obtain

$$p = \int (\mathrm{X}\,d\,x + \mathrm{Y}\,d\,y + \mathrm{Z}\,d\,z);$$

or, as the integration may always be effected, if $\varphi\,(x, y, z)$ represent the integral

$$p = \varphi\,(x, y, z).$$

When p is made constant, this equation will determine a level surface, which, in the simple hypothesis of this problem, is a curve susceptible of only one form for every value assigned to p. Now if p passes through all gradations from its maximum value at the centre of the mass to zero at the upper surface, all the level surfaces will be ascertained; this determines the form of the mass; and the equilibrium follows from the consideration that the level surfaces at all depths are determinate curves.

Of this problem an example is added, which is not undeserving of notice on its own account.

Example.—To determine the figure of equilibrium of an incompressible fluid at liberty, the particles being supposed to attract one another with a force proportional to the distance, at the same time that they are urged by a centrifugal force caused by revolving about an axis.

Solution.—On account of the mutual attraction of the particles it may at first be surmised, that the problem here proposed does not come under the head of which it is given as an example. It does not immediately appear that the forces urging a particle depend entirely on the place of the particle, and are explicit functions of its co-ordinates. Such, however, is the case, owing to a property peculiar to the supposed law of attraction, which NEWTON has demonstrated in the 88th proposition of the first book of his Principia. The property alluded to consists in this, that the resultant of the accumulated attractions of the mass upon a particle is directed to the centre of gravity of the mass, and is the same as it would be if the whole attracting matter were collected in that point. Of this a succinct investigation is as follows.

The origin of the co-ordinates being at the centre of gravity of the whole body of fluid, let x, y, z denote the co-ordinates of an attracted particle, and x', y', z' those of $d\,m$, an element of the mass; the attraction of $d\,m$ upon the particle at the distance f will be $f\,d\,m$; and as the cosines of the angles which f makes with x, y, z are

$$\frac{x - x'}{f},\ \ \frac{y - y'}{f},\ \ \frac{z - z'}{f},$$

the partial attractions of $d\,m$ in the directions in which x, y, z decrease, will be

$$d\,m\,(x - x'),\quad d\,m\,(y - y'),\quad d\,m\,(z - z');$$

and by summing the attractions of all the elements, the partial attractions of the whole mass in the same directions will be

$$x \int d\,m - \int x'\,d\,m,\quad y \int d\,m - \int y'\,d\,m,\quad z \int d\,m - \int z'\,d\,m.$$

But by the property of the centre of gravity,

$$\int x' \, dm = 0, \int y' \, dm = 0, \int z' \, dm = 0;$$

wherefore the attractions of the whole mass upon the particle in the same directions as before, will be

$$x \times m, \quad y \times m, \quad z \times m.$$

Now the resultant of these forces is directed to the origin of the co-ordinates, and is equal to $\sqrt{x^2 + y^2 + z^2} \times m$, which is NEWTON's proposition.

Having proved that the example comes under the foregoing problem, it is next to be observed, that the axis of rotation, supposed parallel to x, will pass through the centre of gravity of the mass: for that point must be at rest by the action of all the forces. Assuming $m \times 1$ to represent the central force, and $\epsilon \times m \times 1$ to denote the centrifugal force at the distance 1 from the axis of rotation; according to what has been shown, the central forces on a particle will be

$$m \times x, \quad m \times y, \quad m \times z;$$

and the centrifugal forces

$$\epsilon \times m \times y, \quad \epsilon \times m \times z;$$

wherefore, by equation (A.),

$$X = m \times x, \quad Y = m \times (1 - \epsilon) y, \quad Z = m \times (1 - \epsilon) z,$$

$$\text{Const.} = x^2 + (1 - \epsilon) . (y^2 + z^2);$$

so that all the level surfaces are elliptical curves; and the figure of equilibrium of the fluid is an oblate spheroid.

The radius of the equator is to the polar semi-axis as 1 to $\sqrt{1 - \epsilon}$; and if $\epsilon = \frac{1}{289}$, as in the case of the earth, the proportion is 578 to 577, agreeing with what HUYGHENS found.

PROBLEM II.

To determine the figure of equilibrium of a homogeneous fluid at liberty, the particles attracting one another in the inverse proportion of the square of the distance, at the same time that they are urged by a centrifugal force caused by revolving about an axis.

Solution.—If we adopt for the unit of mass a sphere of the given fluid having its radius equal to a, the attractive force at the surface of the sphere will be $g \times a$, the value of g being the same for all spheres of the same matter; and if the time of one entire revolution about the axis of rotation be denoted by T, the centrifugal force at the surface of the sphere will be $\frac{4\pi^2}{T^2} \times a$; so that the known quantity

$$\epsilon = \frac{4\pi^2}{T^2} \cdot \frac{1}{g}$$

will denote the centrifugal force estimated in parts of the attractive force.

The origin of the co-ordinates being at the central point, or point of maximum-pressure, let x, y, z represent the co-ordinates of a particle of the fluid, x being parallel to the axis of rotation; and put P, Q, R for the attractions of the whole mass upon the particle in the respective directions of x, y, z: the centrifugal force is equal to $\epsilon \times \sqrt{y^2 + z^2}$ at the distance $\sqrt{y^2 + z^2}$ from the axis of rotation; and the partial forces parallel to y and z, are therefore $\epsilon \times y$ and $\epsilon \times z$: wherefore, taking the total forces acting in the respective directions of x, y, z, we shall have, according to the equation (A.),

$$\mathrm{X} = \mathrm{P}, \quad \mathrm{Y} = \mathrm{Q} + \epsilon y, \quad \mathrm{Z} = \mathrm{R} + \epsilon z,$$

$$p = \int (\mathrm{P}\,dx + \mathrm{Q}\,dy + \mathrm{R}\,dz) + \frac{\epsilon}{2}(y^2 + z^2), \quad . \quad . \quad . \quad . \quad (\mathrm{M.})$$

which equation must be verified by every level surface the upper surface included; p being constant in every level surface, and equal to zero in the upper surface.

Now the equation just found is not sufficient to solve the problem: first, because all the forces that act upon the particles in a level surface, and tend to change its figure, are not taken into account; secondly, because the equation is indeterminate, and incapable of ascertaining a level surface which admits of only one form.

In regard to the first point it is to be observed, that the pressure upon a level surface is the effect of all the forces that urge the particles of the exterior fluid; and in the present instance a part of these forces is the attraction of the fluid within the level surface. But, as action is always attended with reaction, if the fluid within the level surface attract the fluid on the outside and cause it to press, the fluid on the outside will react, and, by its attraction, urge the particles within the level surface to move from their places. It is not necessary to investigate in what manner all the particles within a level surface are acted upon by the exterior fluid: it is sufficient to consider the forces acting upon the particles in the surface itself; because the form of equilibrium of the mass will be ascertained, when the figure of every level surface is determined. Now the nature of a level surface consists in this, that the resultant of all independent forces urging a particle in it, must be perpendicular to it. Wherefore if, as before, x, y, z represent the co-ordinates of a particle in a level surface; and if P', Q', R' denote the partial attractions parallel to x, y, z, of the stratum of fluid exterior to the level surface, we must have this equation,

$$\mathrm{P}'\,dx + \mathrm{Q}'\,dy + \mathrm{R}'\,dz = 0,$$

which expresses the condition that the resultant of the attractions upon the particle is perpendicular to the surface; and in order that the same thing may be true of every point in the surface, we must have the equation

$$\text{const.} = \int (\mathrm{P}'\,dx + \mathrm{Q}'\,dy + \mathrm{R}'\,dz) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{N.})$$

The problem is completely solved by the two equations (M.) and (N.). These equations together take into account all the forces tending to move a particle in a level surface; and what is wanting in the equation (M.) for giving a determinate figure to any such surface, is supplied by the equation (N.).

If we apply the foregoing solution to the oblate elliptical spheroid, it will immediately appear, according to what is proved in the Principia, Lib. 1. Prop. 91. Cor. 3., that the equation (N.) is verified by any surface similar and concentric to the surface of the spheroid. A further simplification arises from the same property; for we may substitute the attraction of the matter within the interior surface, for the attraction of all the matter of the spheroid: so that the solution of the problem is reduced to the single condition of finding an elliptical surface to which the resultant of the attractive and centrifugal forces shall be perpendicular.

It may not be here improper to draw attention to the difference between the analysis of a problem, and its synthetic demonstration. In an analysis it is necessary to mount up to the essential principles of a problem, which always occupy a prominent place in the investigation; whereas a synthetic demonstration may proceed on properties previously investigated, and may be read and understood, although the essential grounds of the problem may never be brought into view. There is no doubt that it is the property cited above from the Principia, which makes the elliptical spheroid, exclusively of all other figures, the form of equilibrium of a homogeneous mass of fluid revolving about an axis; yet of this property no mention is made in MACLAURIN's demonstration. Nay, it has been contended on high authority that the property in question is merely accidental, and not essential to the equilibrium*. A little patience to have traced the property on which MACLAURIN's reasoning rests to its ultimate foundation, would have shown that, however the processes of investigation may be varied, they all originate from one source†.

Having now found the equations for determining *à priori* the figure of equilibrium of an incompressible fluid revolving about an axis, it remains to solve these equations.

PROBLEM III.

To solve the equations of the last problem.

Solution.—These equations are not easily solved without complicated calculations, at least if we proceed by direct methods.

The co-ordinates of a particle in a level surface being x, y, z the partial attractions parallel to x, y, z of the whole mass upon the particle, are represented in the equation (M.) by P, Q, R: and in the equation (N.), P′, Q′, R′ are the like partial attractions of the exterior stratum upon the particle: wherefore if P″, Q″, R″ denote the like partial attractions of the body of fluid within the level surface upon the particle, we shall have to solve these two equations,

$$\text{const.} = \int (\mathrm{P}'' dx + \mathrm{Q}'' dy + \mathrm{R}'' dz) + \frac{f}{2}(y^2 + z^2) \quad . \quad . \quad . \quad . \quad (\mathrm{M}'.)$$

$$\text{const.} = \int (\mathrm{P}' dx + \mathrm{Q}' dy + \mathrm{R}' dz) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{N}.)$$

the sum of which is the equation (M.).

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* POISSON, Traité de Mécanique, No. 593.

† Vide Note, p. 248.

Let $d\mu$ be a molecule of the body of fluid within the level surface; x', y', z' the co-ordinates of $d\mu$; f the distance of $d\mu$ from the attracted particles in the level surface: then

$$f = \sqrt{(x-x')^2 + (y-y')^2 + (z-z')^2}.$$

The direct attraction of $d\mu$ upon the attracted particle is

$$\frac{d\mu}{f^2}.$$

the cosines of the angles which f makes with x, y, z, are respectively

$$\frac{x-x'}{f}, \frac{y-y'}{f}, \frac{z-z'}{f}:$$

wherefore the partial attractive forces parallel to x, y, z, acting upon the particle in the level surface in the directions in which the co-ordinates decrease, will be

$$d\mu \cdot \frac{x-x'}{f^3}, \quad d\mu \cdot \frac{y-y'}{f^3}, \quad d\mu \cdot \frac{z-z'}{f^3};$$

or, which are equivalent,

$$-d\mu \cdot \frac{d \cdot \frac{1}{f}}{dx}, \quad -d\mu \cdot \frac{d \cdot \frac{1}{f}}{dy}, \quad -d\mu \cdot \frac{d \cdot \frac{1}{f}}{dz}:$$

and, observing that x, y, z, are independent of $d\mu$, the like partial forces of all the molecules of the mass of fluid within the level surface, may be thus expressed:

$$-\frac{d \cdot \int \frac{d\mu}{f}}{dx}, \quad -\frac{d \cdot \int \frac{d\mu}{f}}{dy}, \quad -\frac{d \cdot \int \frac{d\mu}{f}}{dz},$$

the integral extending to all the molecules within the level surface. Now these forces are the same in quantity, but have contrary directions to the forces represented by P″, Q″, R″ in the equation (M′.); so that by substituting and then integrating, that equation will be thus transformed:

$$\text{const.} = \int \frac{d\mu}{f} + \frac{\varepsilon}{2}(y^2 + z^2).$$

Now the inspection of this equation is alone sufficient to show that, if it be verified in one curve surface, it will be verified in every curve surface similar and similarly posited about the central point. For assume two attracted points similarly situated in two such similar surfaces; divide the matter within the surfaces into the same number of infinitesimal molecules proportional to the masses within the surfaces; then, taking any molecules similarly situated with respect to the attracted points, the differential

$$\frac{d\mu}{f}$$

will be proportional to the square of the linear dimensions of the surfaces. This is evident; for the numerators are as the masses, or in the triplicate ratio of the linear dimensions; and f representing similar lines of the surfaces, the denominators are simply as the linear dimensions. Wherefore the integral

$$\int \frac{d\mu}{f},$$

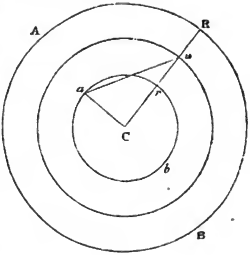
extended to all the molecules within the surfaces, will be proportional to the square of the linear dimensions. Further, because the attracted points are similarly placed in the two surfaces, their co-ordinates will be similar lines; consequently

$$y^2 + z^2$$

will be as the squares of the linear dimensions of the surfaces. From what has been proved we learn that, for attracted points similarly situated in the surfaces, C will vary from one surface to another proportionally to the square of the linear dimensions of the surfaces; wherefore if C be constant at all the points of any one surface, it will be constant at all the points of every surface similar and similarly posited about the central point.

The solution of the equation (M′.) with respect to all the level surfaces, is now reduced to the verification of that equation by the upper surface of the fluid. When this condition is fulfilled, all the interior level surfaces, it has been demonstrated, are similar to the upper surface, and similarly posited about the central point.

We come next to turn our attention to the equation (N). Let A R B represent the surface of the fluid in equilibrium; C the central point; $a\,r\,b$ a level surface similar to A R B, and similarly situated about C; further, a being an attracted point in the level surface, and u a point of the stratum between the two surfaces, put x, y, z for the co-ordinates of a, and x', y', z' for those of u; then f being the distance from a to u, we shall have



$$f = \sqrt{(x-x')^2 + (y-y')^2 + (z-z')^2}.$$

Let $d\mu'$ represent a molecule of the stratum at the point u; the direct attraction of $d\mu'$ upon a particle at a, will be

$$\frac{d\mu'}{f^2}:$$

and by proceeding as before it will be found that the partial forces parallel to x, y, z, caused by the attractions of all the molecules of the stratum upon the particle, and estimated in the directions in which the co-ordinates increase, are as follows:

$$\frac{d\,.\int\frac{d\mu'}{f}}{dx}, \quad \frac{d\,.\int\frac{d\mu'}{f}}{dy}, \quad \frac{d\,.\int\frac{d\mu'}{f}}{dz}.$$

Now these are the forces represented by P′, Q′, R′ in the equation (N.): so that, by substituting and integrating, that equation will be changed into this which follows:

$$\text{const.} = \int \frac{d\mu'}{f},$$

the integral extending to all the molecules of the stratum.

The co-ordinates at the points a and u, are thus expressed:

$$\begin{array}{ll}
\mathrm{C}\,a = r = \sqrt{x^2 + y^2 + z^2} & \qquad \mathrm{C}\,u = s = \sqrt{x'^2 + y'^2 + z'^2} \\
x = r\cos\theta & \qquad x' = s\cos\theta' \\
y = r\sin\theta\cos q & \qquad y' = s\sin\theta'\cos q' \\
z = r\sin\theta\sin q & \qquad z' = s\sin\theta'\sin q',
\end{array}$$

in which formulas, θ, θ' are the angles that r and s make with the axis of rotation; aud q, q' the angles that determine the position of the projections of r and s upon the plane of $y\,z$. By substituting the values of the co-ordinates we obtain

$$f = \sqrt{s^2 - 2\gamma r\,.\,s + r^2},$$
$$\gamma = \cos\theta\cos\theta' + \sin\theta\sin\theta'\cos(q - q').$$

If the variables s, θ', q' change to $s + d\,s$, $\theta' + d\,\theta'$, $q' + d\,q'$, the three small lines $d\,s$, $s\,d\,\theta'$, $s\sin\theta'\,d\,q'$, will be perpendicular to one another; and, as the density is unit, we shall have

$$d\,\mu' = s^2\,d\,s\,.\,d\,\theta'\,d\,q'\sin.\theta'.$$

The foregoing values being substituted, this result will be obtained,

$$\int \frac{d\,\mu'}{f} = \iint d\,\theta'\,d\,q'\sin\theta' \int \frac{s^2\,d\,s}{\sqrt{s^2 - 2\gamma r\,.\,s + r^2}},$$

the integrations extending from $\gamma = 1$ to $\gamma = -1$; and from $\theta' = 0$, $q' = 0$, to $\theta' = \pi$, $q' = 2\pi$; and from $s = r'$, to $s = \mathrm{R}$, r' and R being the values of s at the interior and upper surfaces.

Since r' and R have the same ratio in every position of the line C r' R, we may put $s = (1 + \alpha) \times r'$, $d\,s = d\,\alpha \times r'$; by which substitutions the last equation will become,

$$\int \frac{d\,\mu}{f} = \iint d\,\theta'\,d\,q'\sin\theta'\,.\int (1 + \alpha)\,d\,\alpha\,.\,\frac{r'^2}{\sqrt{1 - 2\gamma\frac{r}{r'(1+\alpha)} + \frac{r^2}{r'^2(1+\alpha)^2}}},$$

the quantity α being constant at all the points of an exterior surface similar to the level surface, but varying from one exterior surface to another. The radical quantity must next be expanded in a series of the powers of $\frac{r}{r'(1+\alpha)}$, viz.

$$1 + \mathrm{C}^{(1)}\cdot\frac{r}{r'(1+\alpha)} + \mathrm{C}^{(2)}\cdot\frac{r^2}{r'^2(1+\alpha)^2} + \mathrm{C}^{(3)}\cdot\frac{r^3}{r'^3(1+\alpha)^3} + \&c.,$$

the coefficients being determined by the well-known formula,

$$\mathrm{C}^{(i)} = \frac{1}{2^i} \cdot \frac{d^i (\gamma^2 - 1)}{1 . 2 . 3 \ldots i \times d\gamma^i}.$$

This expansion is admissible; for $(1 + \alpha)\, r'$ being the radius of a surface exterior to the level surface, $\frac{r}{r'(1 + \alpha)}$ will be less than unit, and the series will always converge. By substituting the series, we get,

$$\begin{aligned}
\int \frac{d\mu'}{f} = & \int_0^\alpha (1 + \alpha)\, d\alpha \,.\iint d\theta'\, dq' \sin\theta' \,.\, r'^2 \\
& + r \,.\, \alpha \iint d\theta'\, dq' \sin\theta' \,.\, r'\, \mathrm{C}^{(1)} \\
& + r^2 \int_0^\alpha \frac{d\alpha}{1 + \alpha} \cdot \iint d\theta'\, dq' \sin\theta' \,.\, \mathrm{C}^{(2)} \\
& + r^3 \int_0^\alpha \frac{d\alpha}{(1 + \alpha)^2} \cdot \iint d\theta'\, dq' \sin\theta' \,.\, \frac{\mathrm{C}^{(3)}}{r'} \\
& \quad \vdots \\
& r^i \int_0^\alpha \frac{d\alpha}{(1 + \alpha)^{i-1}} \cdot \iint d\theta'\, dq' \sin\theta' \,.\, \frac{\mathrm{C}^{(i)}}{r'^{(i-2)}}.
\end{aligned}$$

As $\int \frac{d\mu'}{f}$ must have the same value at all the points of the level surface, it must be independent of r, which varies with the position of the attracted point; and hence we learn that all the terms of the foregoing series containing r, or any power of r, must be separately equal to zero. This brings the question to the two following equations: first,

$$\int \frac{d\mu'}{f} = \int_0^\alpha (1 + \alpha)\, d\alpha \,.\iint d\theta'\, dq' \sin\theta' \,.\, r'^2,$$

which ascertains the quantity of $\int \frac{d\mu'}{f}$, resulting from the attraction of a stratum contained between the similar surfaces, of which the radii are r' and $r' . (1 + \alpha)$, and such that $\int \frac{d\mu'}{f}$ has the same value for all positions of the attracted point in the level surface; secondly, the equation

$$0 = \iint d\theta'\, dq' \sin\theta' \cdot \frac{\mathrm{C}^{(i)}}{r'^{(i-2)}}, \quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad (\mathrm{O.})$$

for all values of i from 1 to ∞, which determines the expression of r', and the nature of the spheroid. It now only remains to solve this equation.

Whatever figure the spheroid sought may be supposed to have, the equation of its surface will be a function of the three co-ordinates of a point of that surface: and the expression of a radius r' deduced from that equation, can only be a function of the three quantities

$$\cos\theta', \quad \sin\theta' \cos q', \quad \sin\theta' \sin q', \quad \text{or } a', b', c',$$

which determine the position of r'. We may suppose that r', or any power of r' positive or negative, is either exactly or approximately a rational function of a', b', c'; and the terms of every such function may be so arranged as to come under this form of expression:

$$U^{(0)} + U^{(1)} + U^{(2)} \ldots\ldots + U^{(n)},$$

$U^{(0)}$ being a constant, and $U^{(n)}$ representing generally a homogeneous function of a', b', c' of n dimensions. By means of this notation we obtain the following theorems:

$$\text{I.} \quad 0 = \int_0^{\pi} \int_0^{2\pi} d\theta'\, dq' \sin\theta' \,.\, U^{(n)} C^{(i)}$$

in all cases when n is less than i:

$$\text{II.} \quad 0 = \int_0^{\pi} \int_0^{2\pi} d\theta'\, dq' \sin\theta'\, U^{(n)} C^{(i)}$$

when n is an even number, and i an odd number.

These theorems relate to a branch of analysis that has been much cultivated; and as they are easily deduced from well-known properties, the demonstrations are omitted for the sake of abridging.

According to what has been said,

$$r' = U^{(0)} + U^{(1)} + U^{(2)} \ldots\ldots U^{(n)};$$

and if we make $i = 1$, $C^{(1)} = \gamma$, the equation (O.) will become

$$0 = \int\int d\theta'\, dq' \sin\theta' \,.\, \{\gamma\, U^{0} + \gamma\, U^{(1)} + \gamma\, U^{(2)} \ldots\ldots + \gamma\, U^{(n)}\}.$$

Now, by the second theorem, all the terms in which n is an even number will be zero; and hence, in order to make the whole expression zero, all the other terms in which n is an odd number must be exterminated. Thus the value of r' that will verify the equation (O.) is as follows:

$$r' = U^{0} + U^{(2)} + U^{(4)} \ldots\ldots + U^{(2n)}.$$

Every power of r' positive or negative will be of the like form, so that

$$\frac{1}{{}^{i-2}} = U^{0} + U^{(2)} + U^{(4)} \ldots\ldots + U^{(2n)};$$

and, by the second theorem, this expression will verify the equation (O.) in all cases when i is an odd number.

As the equation (O.) does not contain r' when $i = 2$, it is obviously verified in that case. When $i = 4$ we have

$$\frac{1}{r'^2} = \mathrm{U}^0 + \mathrm{U}^{(2)} + \mathrm{U}^{(4)} +, \&c.;$$

and the equation (O.) will take this form,

$$0 = \int\!\!\int d\,\theta'\, d\,q' \sin\theta' \,.\, \{\mathrm{U}^0\,\mathrm{C}^{(4)} + \mathrm{U}^{(2)}\,\mathrm{C}^{(4)} + \mathrm{U}^{(4)}\,\mathrm{C}^4 +, \&c.\};$$

of which expression the two first terms are zero by the first theorem; but as the succeeding terms have all determinate values, they must be cancelled, which limits the expression of $\frac{1}{r'^2}$ that will verify the equation (O.) to

$$\frac{1}{r'^2} = \mathrm{U}^{(0)} + \mathrm{U}^{(2)}.$$

We have now only to inquire whether the expression of $\frac{1}{r'^2}$ thus found will verify the equation (O.) in all cases when i is an even number greater than 4: now

$$\int\!\!\int d\,\theta'\, d\,q' \sin\theta' \frac{\mathrm{C}^{(i)}}{r'^{(i-2)}} = \int\!\!\int d\,\theta'\, d\,q' \sin\theta' \,.\, \mathrm{C}^i\, (\mathrm{U}^0 + \mathrm{U}^{(2)})^{\frac{i}{2}-1}:$$

and i being any even number, if the binomial quantity be expanded and arranged in homogeneous functions, it will be of this form,

$$\mathrm{U}^{(0)} + \mathrm{U}^{(2)} + \mathrm{U}^{(4)} \ldots + \mathrm{U}^{i-2},$$

the substitution of which will produce a series of terms, every one of which will vanish by the first theorem.

The foregoing investigation proves that every figure capable of fulfilling the conditions required for the equilibrium of an incompressible fluid subject to the law of attraction that prevails in nature, and revolving about an axis, is comprehended in the formula

$$\frac{1}{r'^2} = \mathrm{U}^{(0)} + \mathrm{U}^{(2)}.$$

Taking the most general expression of $\mathrm{U}^{(2)}$, which stands for a homogeneous expression of two dimensions, of $\cos\theta'$, $\sin\theta' \cos q'$, $\sin\theta' \sin q'$, or a', b', c'; and observing that the constant

$$\mathrm{U}^0 = \mathrm{U}^0 \,.\, a'^2 + \mathrm{U}^0 \,.\, b'^2 + \mathrm{U}^0 \,.\, c'^2,$$

may be blended with the expression of $\mathrm{U}^{(2)}$, we shall have

$$\frac{1}{r'^2} = \mathrm{A}\,a'^2 + \mathrm{B}\,b'^2 + \mathrm{C}\,c'^2 + \mathrm{D}\,a'\,b' + \mathrm{E}\,a'\,c' + \mathrm{F}\,b'\,c':$$

and as $r'\,a'$, $r'\,b'$, $r'\,c'$ are the co-ordinates of a point in the surface of the fluid, we obtain this general equation:

$$1 = \mathrm{A}\,x'^2 + \mathrm{B}\,y'^2 + \mathrm{C}\,z'^2 + \mathrm{D}\,x'\,y' + \mathrm{E}\,x'\,z + \mathrm{F}\,y'\,z',$$

which is that of an ellipsoid, the co-ordinates being parallel to any three diameters intersecting at right angles. This general equation is modified by the rotatory motion; for it is easy to prove that the axis about which the fluid revolves, or the diameter parallel to the co-ordinate x', must be perpendicular to the surface of the fluid, and consequently it must be one of the axes of the ellipsoid: and as nothing hinders from assuming the other two axes for the diameters to which the co-ordinates y and z are parallel, the foregoing equation will take this more simple form:

$$1 = \mathrm{A}\,x'^2 + \mathrm{B}\,y'^2 + \mathrm{C}\,z'^2 = \frac{x'^2}{k^2} + \frac{y'^2}{k'^2} + \frac{z'^2}{k''^2},$$

the three semi-axes of the ellipsoid being k, k', k'', of which k is the axis of rotation. Thus the ellipsoid comprehends every possible figure of equilibrium, the rotatory motion being performed about one of the axes. When the centrifugal force is given, the particular figure of equilibrium is found by making the resultant of the attractive and centrifugal forces perpendicular to the surface of the ellipsoid.

Note of Mr. Ivory *relating to the correcting of an error in a Paper printed in the Philosophical Transactions for* 1838, pp. 57, &c.

In the paper referred to* it is said, "Let V stand for the integral in the equation (7.), and supposing that p and τ^2 vary so as always to satisfy that equation, we shall have

$$\frac{d\,\mathrm{V}}{d\,p}\,d\,p + \frac{d\,d\,\mathrm{V}}{\tau\,d\,\tau} = 0."$$

Now the error alluded to consists in having given a wrong sign to the differential $\frac{d\,\mathrm{V}}{\tau\,d\,\tau}$, the value of which, as has been said, is modified, in the question under consideration, by the equation (7.). This mistake vitiates the concluding part of the paper in pp. 63, 64, 65, as far as relates to the limits of the quantities p and τ^2. M. Liouville has done the author of the paper the honour of noticing and correcting the mistake in his *Journal de Mathématiques for April* 1839. When the sign of the differential is rightly ascertained, the analysis pursued in the paper leads to a simple determination of the limits sought, as this Note will prove, the propriety of printing which in the Philosophical Transactions is submitted to the Council.

For the sake of abridging expressions, put

$$\Delta^2 = (1 + p\,x^2)^2 + \tau^2\,x^2, \quad \mathrm{P}^2 = (1 + p)^2 + \tau^2;$$

then

$$(1 - x^2)\,(1 - p^2\,x^2) = \Delta^2 - \mathrm{P}^2\,x^2.$$

By substituting this value in equation (7.),

$$\mathrm{V} = \int_0^1 \frac{x^2\,d\,x}{\Delta} - \mathrm{P}^2 \int_0^1 \frac{x^4\,d\,x}{\Delta^3},$$

$$\frac{d\,\mathrm{V}}{\tau\,d\,\tau} = -\,3 \int_0^1 \frac{x^4\,d\,x}{\Delta^3} + 3\,\mathrm{P}^2 \int_0^1 \frac{x^6\,d\,x}{\Delta^5};$$

and hence

$$\mathrm{V} + \frac{\mathrm{P}^2}{3}\cdot\frac{d\,\mathrm{V}}{\tau\,d\,\tau} = \int_0^1 \frac{x^2\,d\,x}{\Delta}\left(1 - \frac{\mathrm{P}^2\,x^2}{\Delta^2}\right)^2;$$

which proves that the same values of p and τ^2 that make $\mathrm{V} = 0$, will necessarily make $\frac{d\,\mathrm{V}}{\tau\,d\,\tau}$ positive.

Further, we have

$$\frac{d\,\mathrm{V}}{d\,p} = -\int_0^1 \frac{x^4\,(1 - x^2)\,d\,x}{\Delta^5}\left\{(3 + 2\,p - p^2\,x^2)\,(1 + p\,x^2) + 2\,p\,\tau^2\,x^2\right\};$$

from which it follows that, whatever positive number τ^2 stands for, $\frac{d\,\mathrm{V}}{d\,p}$ is negative for all values of p that make $3 + 2p - p^2$ positive, that is, for all values of p less than 3.

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* Philosophical Transactions, 1838, p. 63.

The function V is positive when $p = 1$; it is zero when $p = l^2$ and $\tau^2 = 0$. And, if we suppose that p decreases from l^2 to 1, while τ^2 increases from 0 to ∞, the differential equation (A.) will in no instance be verified; because, according to what has been shown, both the terms of the equation will be positive between the limits mentioned. Thus there is no value of p less than l^2 that will verify the equation (7.).

It is proved in the paper (p. 62) that for every assumed value of τ^2, there is a positive value of p, that will verify the equation (7.); and, as it has now been shown that the values of p which verify that equation cannot be less than l^2, they must be all greater than l^2.

Further, in the differential equation (A.), $\frac{dV}{dp}$ cannot be zero; because, τ^2 increasing without limit, $\frac{V}{\tau d\tau} \cdot \tau d\tau$ is essentially positive. Now, for all values of p between l^2 and 3, $\frac{dV}{dp}$ is negative; wherefore the same function will continue to be negative in the equation (A.) for all values of p and τ^2: and as $\frac{dV}{dp} dp$ is also negative, dp must be positive, so that p will increase above l^2 without limit.

If the sign of $\frac{dV}{dp}$ be changed, the result will be positive; and hence, observing that x is contained between 0 and 1, we obtain a condition between any two values of p and τ^2 that satisfy the equation (7.), namely, the expression

$$(3 + p - p^2)(1 + p) + 2p\tau^2$$

must be a positive quantity, or, which is the same thing,

$$\tau^2 > (p^2 - p + 3) \cdot \frac{1 + p}{2p}.$$

J. IVORY.

July 25, 1839.

XVI. *Report of a Geometrical Measurement of the Height of the Aurora Borealis above the Earth. By the Rev.* JAMES FARQUHARSON, *LL.D. F.R.S., Minister of the Parish of Alford. Communicated by Major* SABINE, *R.A. F.R.S. &c.*

Received May 30,—Read June 20, 1839.

WHEN, in 1833, I observed the "instructions for observers of the aurora borealis," circulated by the British Association for the Advancement of Science, I became desirous of aiding in the attainment of the objects the Association have in view; the chief of which are, the determination by geometrical measurement of the height of the meteor above the earth, and of the altitude and azimuth of the point to which the streamers seem to converge, and which has been named the centre of the corona.

The full accomplishment of my purpose has been delayed by impediments, the chief of which will be referred to in the progress of this Report. I have, however, at length obtained such results as I conceive will be deemed of importance, and I beg leave respectfully to present them to the notice of the Royal Society.

I had soon an opportunity for determining, with sufficient accuracy, the altitude and azimuth of the point in the heavens to which the streamers seem to converge; and as this constitutes an important element in enabling us to form a clear conception of the whole definite arrangement and progress of the meteor, and of the extent of reliance to be placed on the method afterwards had recourse to for measuring the height above the earth, and as that distinguished observer, Major SABINE, has since determined the dip of the magnetic needle at this place, which agrees with the angle of altitude of the point of convergence of the streamers, I shall proceed, first, to detail, from my notes made at the time, my observations on this point.

On the 29th December, 1833, during the progress of a hard gale at nearly due west, temperature 42° FAHR., there occurred here an aurora borealis, more nearly than any other I have seen, like the one seen by me here on the 29th of September, 1828, contemporaneously with a luminous arch seen by DAVIES GILBERT, Esq. P.R.S., at Rosemorran in Cornwall, as described by me in a letter to the President, which was honoured with a place in the Philosophical Transactions*. The chief difference of the two consisted in there being, on the 29th of December, 1833, many more arches of great length from east to west, well-defined, and separated, when in the plane of the magnetic dip, by clear lanes, from all the other arches and lights in other parts of the sky.

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* Philosophical Transactions, 1829, pp. 103–120.

At 6 P.M. innumerable groups of brilliant streamers, and nebulous patches of light, appeared in almost all parts of the sky, but arranged into narrow arches, with clear lanes between them, near the plane of the magnetic dip. The sky was clear of clouds, with the exception of one cloud of low elevation at south-east. When the meteor was first noticed, there was one narrow distinct arch, almost continuous from the eastern to the western horizon, a little north of, and parallel to, the plane of the magnetic dip. This arch made a rapid progress southward, always preserving its parallelism to the plane of the magnetic dip, and in four or five minutes passed into that plane, where its vertex presented the appearance named by observers a corona, well-defined and brilliant. The appearance of the corona is that of innumerable pencils, or brushes of light radiating in all directions from a centre. In the present instance the pencils pointing northward and southward were short, ending at the edges of the arch, which was only 4° or 5° broad; and those that pointed eastward and westward became blended with the streamers which formed its east and west ends. A small space in the centre of the corona had a nebulous, rather than radiated appearance. This was only about one degree broad, but prevented the determination of the centre, excepting by approximation within half a degree.

At the moment when, in the southward progress of the arch, the centre of the corona became best defined, I had a good resource at hand for determining its place in the heavens, approximately, within such limits as the nebulous centrical light allowed. This consisted in marking, on an outside stair rail, the precise point from which the centre of the corona was exactly seen, over an extreme projecting point on one of the corners of the Manse here.

The distinct arch of the aurora now described, continued to make a rapid progress southward, preserving always a parallelism with its earlier positions, and with the plane of the magnetic dip. The short streamers to the north of the centre of the corona became more shortened; and those to the south of it became proportionally longer, preserving in this way the vertex of the arch of a mean breadth with that of the other parts of it, till at length the corona lost the whole rays that had a northerly direction. The whole arch, as it went on still further towards the south, became gradually enlarged in its lateral dimensions, that is, in breadth from north to south, as I have described other arches to have done, in the letter to the President of the Royal Society above referred to.

My attention was soon withdrawn from this arch by another now approaching the zenith from the north. This was composed of much more brilliant streamers than the former. It was considerably wider from north to south, and less evenly defined at its north and south edges, appearing there fitfully rugged, but quite distinctly separated from the numerous other lights in the sky by lanes clear of any form of the meteor. Expecting that it too would pass southward, and form a corona, when it reached the position where the other had done so, I provided a long straight-edged ruler, to ascertain by it whether the streamers in all parts of the sky were directed to the centre of the corona. The arch soon passed southward into the plane of the magnetic dip, narrowing in breadth till it reached that position, and forming when there a corona, exactly like the former one, the centre of which had the same altitude and azimuth. On looking at the centre of the corona over the straight edge of the ruler at one of its ends, while it was held out at arm's length, and at the same time bringing the other end round to all parts of the sky, it was found that everywhere the streamers were directed lengthwise to the centre of the corona.

This latter arch passed southward as the former had done; and like it also, increased its breadth from north to south in its onward progress from the plane of the dip. It was succeeded by many other arches and fragments of arches, that is, arches cut short in their east and west dimension, all of which behaved themselves in a manner analogous to the two first arches, or to corresponding portions of them. About an hour and a half after the aurora was first seen, the phenomena became faint at all points, and the sky soon after became obscured by clouds.

On the day following these observations, I measured, by means of a small graduated semicircle, with plummet, the angle that the line, joining the mark on the stair rail and the projecting point on the corner of the Manse, made with the horizon, and found it about 72°. Major Sabine afterwards, on the 27th and 29th of August, 1836, by 160 readings of his dipping-needle, determined the dip at this place to be then 72° 19′·5 *. I also laid down a horizontal line in the azimuth of the two points; and having suspended over it, by means of a silk fibre, a horizontal magnetic needle, found that the needle came to rest parallel to the line.

It has been a matter of more difficulty to arrange the means, and find a fit opportunity, to determine the height above the earth, by the method recommended by the British Association, namely, by instrumental measurements of the angular elevations of an arch, made contemporaneously at two stations on the magnetic meridian, sufficiently distant from each other to give a clear and satisfactory parallax.

With the view of effecting this, I entered into arrangements with a gentleman whose residence is about six miles north of this place, and very near its magnetic meridian. There intervenes the ridge of the Coreen hills, extending in length from east and west about ten miles, in breadth from north to south about four miles, and elevated at many points about 1000 feet above the level of the two stations. After looking out for some years for corresponding observations of the same arch, although there was no want of appearances of the aurora, the gentleman and I found that the observed conditions and circumstances were so discrepant, as to prohibit the inference that we had at any time witnessed the same phenomenon. We had agreed that each should observe to the northward of his own station.

In the mean time I obtained information which I deemed highly valuable for determining the locality of the stations that might be next selected. The Rev. John Minto, a native of this parish, and schoolmaster of Clatt, which is on the north side

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* Major Sabine's Observations on the Direction and Intensity of the Magnetic Force in Scotland, 1836.

of the Coreen hills, had frequent occasion to pass between this place and Clatt. His path over the hills is, at its most elevated part, nearly in the line of the magnetic meridian. He informed me that he had passed in this line several times after dark, on occasions when the aurora borealis was visible. At these times he had travelled in the northerly direction only; but after seeing the whole meteor to the northward of his place, while ascending the south side of the highest part of his path, when he reached the valley on the north side, he then saw the aurora wholly to the south, and at such a low elevation as could not be accounted for by the known movement of the meteor itself, without taking into account its apparent change of place occasioned by his own movement northward. This circumstance occurred to him several times; and he became impressed with the conviction, that the place of the meteor on these occasions was the Coreen hills, and that it was at no great elevation above them. Mr. Minto's conclusion coincided with those I have had reason to form, from many observations of the meteor, respecting the lowness of the region in which it is visible; and admitting the conclusions to be correct, it became obvious that the two stations, from whence to determine its height, would be both most expediently chosen on one side of the ridge of hills. This choice of stations was therefore had recourse to, and with a completely satisfactory result, as will be afterwards stated.

But previously to presenting the observations that were made at the selected stations, it seems necessary, for a right appreciation of their value, to make some remarks on an obvious misapprehension that yet seems to be entertained of the arrangement and progress of the aurora. I had shown that its arrangement and progress are definite in relation to the lines of magnetism of the earth, in a paper published in the Edinburgh Philosophical Journal in April, 1823*, and afterwards in the letter addressed to the President of the Royal Society, and published in the Philosophical Transactions, 1829, above referred to. In the latter publication, after describing various appearances of the aurora, I stated that these appearances indicated the following necessary results:—

"1st. That the aurora borealis always presents itself in definite and very curious relations to the lines of magnetism indicated by the needle.

"2nd. That the streamers, in the direction of their length, coincide with the plane of the dip of the needle, or nearly so; and that each individual streamer is, in fact, parallel to the dipping-needle.

"3rd. That they [the streamers] form a thin fringe, stretching often a great way from east to west at right angles to the magnetic meridian.

"4th. That the fringe moves away from the north magnetic pole, by the extinction of streamers at its northern face, and the formation of new ones, contiguous to its southern face.

"5th. That the invariable regularity of its appearance, as seen by many observers, when it comes fully within command of the eye near the zenith, shows the apparent irregularities, when it is seen either more northerly or southerly, to be only optical illusions.

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* Vol. viii. p. 303.

"6th. That the region which it occupies is above, and contiguous to that of the clouds, or that in which they are about to form."

Some terms employed in the "Instructions" of the British Association, and some also employed by M. Arago, in a passage of his printed in the Edinburgh New Philosophical Journal*, show, that these inductions of mine, regarding the arrangement and progress of the meteor, have been misunderstood or not admitted as accurate. In the "Instructions" it is said, "the determination of elevation can scarcely be applied to streamers of aurora, except when some sudden incurvation, or change, occurs which may happen to be noticed at two stations; but the arches are of a less evanescent nature." This obviously implies that the streamers and the arches are two distinct and unconnected branches of the phenomena. The terms of M. Arago imply the same thing. He says, "when in our climates the aurora borealis is complete, when one part of its light pictures on space a well-defined arch, the culminating point of *this arch* is in the magnetic meridian; and its two points of apparent intersection with the horizon are at equal angular distances from the same meridian. When it projects luminous columns, [by which he evidently means streamers,] from different portions of the arch, their point of intersection, called by certain meteorologists the centre of the cupola, is found in the magnetic meridian, and precisely upon the prolongation of the dipping needle." Here too the arches and the streamers are considered as distinct objects. But an *arch* of the aurora is composed of a number of *streamers* grouped and aggregated together within a defined stereometric space, whose bounding planes bear certain relations to the lines of magnetism of the earth. It is this fact that I announced in the Edinburgh Philosophical Journal in 1823, and afterwards in the letter to the President of the Royal Society; and if the arrangement is not yet understood, it may have been owing to a misapprehension of the terminology I employed; or, more probably, to my not having, in the letter to the President, entered into a detail of the varieties of the arches that often present themselves, as I had done in the Edinburgh Journal. I would endeavour now to supply the deficiency, by defining the terms, and entering into some more detail of the varieties, or rather apparent varieties of the arches; for the numerous observations of the aurora I have since made, convince me more fully of the existence of that definite order of its arrangement and progress which I have formerly described, and that it is worthy of being understood, however much I may have hitherto failed clearly to explain it.

The terms *arch* and *streamer* of the aurora, I had used without defining them, because I conceived them to be in common use among observers; and I shall now perhaps be able best to define them, after presenting anew one part of the description of the whole meteor, which I had given in the Edinburgh Philosophical Journal in 1823, which is as follows. "In this latitude, (about 57° 12′ N.) the aurora borealis, on those evenings when it is visible, *generally* first shows itself, after dark, like a bright but circumscribed twilight on the visible horizon, the centre of which is exactly on the northern point of the magnetic meridian. So long as the bright space continues low, its light often nearly resembles the pale blue white light of the real twilight; but varies momentarily by incessant and undefined fits of gleaming and obscuration. By degrees the meteor enlarges itself, rising higher, and extending more from east to west on the horizon. The play of the fitful gleaming light becomes gradually better defined; and the whole luminous space presents the appearance of pencils, or bundles, of rays pointing upwards, and, when viewed in narrow compartments, maintaining a parallelism among themselves, similar to that exhibited by the rays of the sun when he shines through broken clouds athwart a hazy atmosphere. The rays which are on the magnetic meridian are parallel to that line, pointing exactly to the zenith; and those which are considerably to the eastward or westward of that meridian are directed to a point which appears within the limits of 10° [18°] to the southward of the zenith. The bluish white light changes into a beautiful pale green, which when the meteor rises quite above the horizon, as will be afterwards described, becomes tinged at the lower extremity of the rays with blue and violet, and at their upper extremity with yellow and orange. The rays are very various in their intensity of light, as compared with one another; their higher and lower portions also frequently differ from each other in that respect; and the whole appearance of each ray varies incessantly. It now breaks off, and disappears for a considerable space at its higher or lower extremity, and then immediately becomes again luminous to its former extent; now seemingly runs from east to west or from west to east through 5° or 10° or 12°, during the space of a second or two, preserving correctly its parallelism with other rays, which it approaches or passes in its progress; then remains stationary for a second or two, undergoing various changes of vividness; and afterwards disappears instantaneously, to have its place supplied by another ray, created as rapidly as its predecessor was annihilated. This magnificent and beautiful light gradually extends itself towards the south, and at length separates itself from the northern horizon at the point of the magnetic meridian, and forms a flat luminous arch in the northern part of the heavens. The arch still goes on to make progress towards the south, its convex or upper part approaching the zenith, and its concave or lower side becoming more widely separated from the horizon. When it reaches an elevation of about 45°, it presents the appearance of a broad [curved] zone, occupying from north to south the space of from 25° to 35° at its vertex, and having its eastern and western extremities resting on the visible horizon."

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* Vol. xxv. p. 419. I regret that I cannot more particularly indicate M. Arago's publication, having seen the passage only in the Magazine.

In this passage, both the streamers and the arch of the aurora are described. The streamers are the narrow pencils, or bundles, of rays whose upper extremities are directed to that point in the heavens which forms the upper prolongation of the line of the dipping needle. They are severally in a state of incessant extinction and renovation. An arch is the curved zone of light formed by an aggregation or grouping of a great number of streamers into a definite form. Although composed entirely of parts (streamers) which are severally evanescent, yet the perpetual renewal of these within a definite space, or in a definite direction in relation to the place of the extinct ones, renders the arch relatively permanent; and it often continues for several hours, shifting however gradually towards the south.

In continuation of the above passage taken from the Edinburgh Philosophical Journal, I had described the further progress of such an arch as that mentioned in it, and shown, that it passes at length into the plane of the magnetic dip, gradually contracting its breadth, from north to south, as it nearly approaches that plane, the streamers of which it is composed still pointing, in every part of it, to the point which is on the prolongation of the dipping needle; and that, when it at last reaches the plane of the magnetic dip, it forms a narrow zone, seldom exceeding 4° or 5° from north to south, stretching across the heavens at right angles to the magnetic meridian, and composed now, in its east and west ends, of streamers which no longer cross its breadth, as they did while it was in a more northerly position, but are parallel to the zone itself, and therefore pointing east and west. In that journal, I had added to the description of the arrangement and progress of the longer and more complete arches that present themselves, a description of some of the varieties, or shorter arches, which at various times occur, either by themselves, or mingled up with the longer arches. I shall quote from the Edinburgh Philosophical Journal the account I had given of these varieties, because, in the letter to the President of the Royal Society, I neglected to describe them along with the more lengthened arches; and the neglect may have occasioned misapprehension of the whole subject on the part of those who have seen only the letter to the President.

In the Edinburgh Journal, after a description of the more lengthened arch, and its progress, it is added, "such is the order of appearances presented by the aurora borealis, when it is observed under the most favourable circumstances. It is very seldom, however, that all the successive phenomena, now described, have been observed continuously on the same evening; but those observed at any one particular time have always been entirely consistent with the above description; and I shall now enter a little into a detail of the varieties which present themselves. It very frequently happens, that the twilight-like appearance on the northern horizon is all that is visible, and the phenomenon begins and ends with that. In this case the meteor is seldom of long continuance; but during the time that it lasts, the luminous space gradually enlarges itself towards the south. It then gradually disappears; frequently to be succeeded by another, appearing low on the horizon, to enlarge, and afterwards disappear, as its predecessor had done. It happens also, very frequently, that, even when it makes more progress towards the south, it becomes gradually extinct long before it reaches the zenith; for it is liable to a total extinction in every stage of its advancement; but while it does continue, it follows the order above described, presenting the longest pencils of rays when about 45° elevation, and more dense, compact and shorter ones near the zenith. It also very frequently happens that the meteor is suddenly formed high above the horizon, at first by feeble detached rays, becoming quickly more condensed and luminous. But in whatever stage it first begins, the succeeding relative progress is the same as above described. It was chiefly those meteors, which were first formed above the horizon, that were observed to pass over the zenith. Those formed further northward generally disappeared before reaching that point. There is another modification of these appearances, and that is, when the meteor is entirely to the eastward or westward of the magnetic meridian; and this is of not unfrequent occurrence. In this case, the appearance and progress of the whole exactly agree with those of corresponding portions of the above described zone, which is formed when the meteor extends across the magnetic meridian. The extremity of the luminous space which is nearest the magnetic meridian becomes first elevated above the horizon; the pencils of rays are directed, longitudinally, to a point a little south of the zenith; and the meteor moves gradually towards the south, contracting gradually its lateral dimensions as it reaches the prime vertical to the magnetic meridian, where it assumes the appearance of a nearly vertical column of brilliant light, 3° or 4° in diameter, composed of pencils of rays parallel to itself. After passing some degrees to the southward of the prime vertical, the meteor begins to enlarge gradually in width, in an order the reverse of that in which it had become narrowed. Some other apparent irregularities have been at times observed. Thus the pencils of rays have sometimes been seen separated into detached groups; but each group consistent in its appearance and position with those of the other groups, so that, had the spaces between them been filled up, a complete zone, such as above described, would have been formed. A detachment into distinct groups sometimes takes place immediately previous to the disappearance of the meteor; but sometimes also it is not immediately followed by that disappearance, but the zone becomes again complete, or nearly so, at a further stage of the progress southward. But no [real] anomalies have been at any time observed; nothing that is inconsistent with the described order of the phenomena*."

In addition to this detail of the varieties taken from a former description, it seems necessary here to add, that although the most frequent appearance of the meteor consists of one, or a few regular zones behind each other in a north and south direction, of great length from east to west, yet sometimes, as on the evenings of the 29th September 1828, and 29th December 1833, the lengthened zones are accompanied with all the described varieties, placed in the intervals between them. The whole hemisphere is, in such instances, pervaded by the meteor; and, on looking northward or southward, no regularity in the arrangement can be readily distinguished. But on looking up in the plane of the magnetic dip, the described order is there immediately discovered. The meteor there presents the appearance of intermingled longer or shorter narrow belts of fitful light, at right angles to the magnetic meridian, or columns at the east or west, having all clear intervals between them, and all making progress towards the south, preserving at the same time their parallelism with each other. It seems also necessary to add, that the light of the several belts differs greatly in intensity; some being composed of closely crowded and very brilliant streamers, and others of a few feebly-lighted, ill-defined ones.

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* Major SABINE, while here in 1836, and observing the range of the Coreen hills, northward of this place, to run exactly at right angles to the magnetic meridian, suggested that the great regularity of the appearance and progress of the aurora here might be dependent on that circumstance. The suggestion is highly worthy of attention. The natural tendency of the meteor to the described order may be aided by the position of the hills; and hence a peculiar regularity here; but that the tendency is not altogether dependent on the locality, is proved by the crown of a lengthened arch being, in all places in Britain, on the magnetic meridian, and by the narrowness of the zone of light in the plane of the dip, as witnessed and reported by many observers.

It is now apparent, from the description, that the streamers are not phenomena distinct from the arches, but that the latter are just aggregations of the former within certain stereometric spaces, whose bounding planes bear definite relations to the lines of magnetism of the earth. In the letter to the President of the Royal Society I named these aggregations fringes; and, in fact, they resemble long fringes composed of threads, the threads being represented by the individual, nearly vertical streamers, or lengthened narrow straight pencils of rays of light. The length of these filled stereometric spaces, at right angles to the magnetic meridian, is often very many miles, extending over head, when they come to the zenith, from the east to the west horizon, although they are frequently cut short in this dimension. Their depth in a nearly vertical direction, or more properly in the plane of the magnetic dip, is determined by the length of the individual streamers that fill them. This length of the streamers seems to vary considerably; as some of the arches at about 45° elevation may be seen about 20° broad from north to south, and others 30° and upwards. The streamers in the same space, too, are not equal in length among themselves, their inequality in this respect causing the jagged appearance of the north and south edges of the arches; but in this respect also there are great differences, the edges of some arches, especially on very calm evenings, being even and regularly defined. The thickness of the filled spaces from north to south appears to be always much less than their depth in the plane of the magnetic dip. This thickness from north to south is clearly seen when the meteor comes into that plane, in its progress southward. When there, an arch that at 45° elevation may have been 20° or 30° broad from north to south, generally appears no broader than 4° or 5°, or even than 2° or 3° in some cases*. It is the regular southward progress of the meteor that gives opportunity for the determination of the depth and thickness of the spaces filled with streamers. When the meteor is considerably to the north, its depth is more or less directly seen; when in the plane of the dip, its thickness from north to south is seen; and when in that station, also, its progress southwards is seen to be occasioned by the extinction of streamers at its northern face, and the formation of new ones at the southern face; and when it proceeds further south than the plane of the dip, its depth comes again more or less directly into view. It is obvious that the stereometric spaces thus filled with streamers, when of considerable length from east to west, necessarily present to the eye the appearance of arches of light; but the peculiar direction of the streamers gives to the arch, when in the plane of the dip, a very different aspect from what it has in a more northerly or southerly position. I have so frequently seen the northerly arch pass into the plane of the magnetic dip, and assume there a very different appearance, as to leave no room for the suggestion in the "Instructions" of the British Association, that the arches in the two positions may be of different origin.

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* For a long time, I had not observed zones, in the plane of the dip, broader than about the measures here stated; but in later years I have witnessed some very considerably broader. These are, however, not of frequent occurrence, and present a like arrangement and progress with the narrower zones.

We are now prepared to make some observations on M. ARAGO's reference to "luminous columns projected from different points of the arch." Such an appearance is often seen, that is, luminous columns *seeming* to spring out of a lower and regular arch; but it is only seeming; for the columns belong to another arch composed of fewer streamers, more near the place of the observer, and therefore seen at a higher angle of elevation. This is discovered when both the appearances come southward towards the plane of the magnetic dip. Then the columns are seen to be advanced southward of the arch; and there is observed an interval, frequently a wide one, clear of lights, between the two appearances. The space occupied by the *columns* (*streamers*) may become more densely crowded with additional streamers in its onward progress, in which case it will become a well-defined arch, before it reach the plane of the dip; or the streamers may not increase in number, yet, when it gets into that plane, it will be seen as one of those feebly lighted belts composed of few streamers which I have recently described. I have so frequently seen one or other of these results, in the case of columns *seeming* to project from a lower arch, as to leave no doubt that the appearance was an optical illusion, and that the columns were much nearer the eye than the arch.

I trust it will be admitted, that we are now able to decide, under what conditions of its appearance, we shall effect a satisfactory geometrical measurement of the height of the aurora above the earth. No measurement of this kind, it is obvious, can be obtained, as M. ARAGO justly observes in the passage I have referred to, by taking elevations of the centre of the cupola, or corona, when the meteor is in the plane of the magnetic dip. When the meteor is there, each observer sees a centre peculiar to his own station, and which is in the line of the upper prolongation of the dipping needle; just as in a row of persons placed under the key-stones of a bridge of masonry, each one sees a joint of the masonry vertical to himself, and different from the joints over the others. But, as in the case of the bridge, when we remove ourselves quite from beneath it, and to some distance from it, we can then easily determine the height of either the upper or lower parts of the key-stones, by means of a combination of parallaxes, so we may in like manner measure the height of an arch, or fringe, of aurora, which is placed considerably to the northward or southward of the stations we may select for determining the elevations.

But in the case of the aurora there are serious impediments to the process, in the generally fitful unsteadiness of the object, and equally fitful irregularity of its edges. To avoid the latter inconvenience, the British Association direct the angles of elevation to be taken at the lower edge of the arch (which, it is said, is always best defined) at its summit. I have not observed this edge better defined than the other, excepting on the occurrence of a dark mass under the luminous space, which sometimes takes place, as stated in the instructions. On such occasions, however, the lower edge of the luminous space is always comparatively low, and therefore no clear parallax could be obtained from two stations, unless they were very remote from each other. Besides, the dark mass and defined lower edge are of rare occurrence here, not having been seen for several years.

I have observed already, that in very calm weather the upper edge of the arches is sometimes well-defined; and as that edge, generally, in the arch which first appears here in the evening, is clear from interference with subsequent arches, which for the most part appear only under it, I made preparation for taking elevations of such an arch from two stations, by obtaining the favour of the Rev. HUGH M'CONNACH, the schoolmaster of this parish, to make the observations at one of the stations.

The stations were chosen with reference to the aurora being probably immediately over the Coreen hills, as indicated by Mr. MINTO's observations. The Manse here answers, in this view, for one station, being by measurement 15,700 and 12,500 feet distant from the two nearest elevated summits of the hills respectively; the nearest of the two being almost on its magnetic meridian. The other station could not be placed at a sufficient distance on the north side of the Manse for the river Don intervening. It was therefore placed at Hillhead of Kingsford, on the magnetic meridian of the Manse, and 6810 feet distant from it towards the south. A further extension of the base line in this direction was prevented by a rapid descent of ground, a little beyond the south station, which would have placed the observer out of sight of the Coreen hills. Besides, it was considered inexpedient to have the stations more remote from each other, as it seemed very desirable for the observers to meet before commencing their operations, that they might, on trial, discover how nearly they would agree in taking the elevation of the vertex of the arch of the aurora, an object, in the most favourable case that they could anticipate, somewhat undefined.

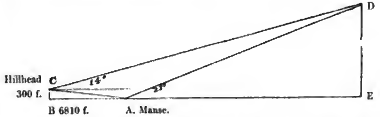
After having looked out a considerable time for a proper display of the meteor, and allowed many to pass which had not the requisite conditions, at length, on the 20th of December, 1838, a little after 6 P.M., a very low complete arch, having a well-defined and regular upper edge, presented itself over the Coreen hills. There was a dead calm at the time, with a clear sky; thermometer 39° FAHR. The arch rose slowly upwards, becoming more bright, and promising to be of some continuance. Mr. M'CONNACH and I met soon after its appearance; and on repeated trials with two small quadrants with plummets, attached to poles for fixing in the ground, found we agreed in determining the angle of elevation of the vertex of the upper edge of the arch within the limits of half a degree. We then adjusted our watches to the same time; and Mr. M'Connach went with an assistant to Hillhead of Kingsford, to take elevations at every five minutes by the watch, while I remained at the Manse, to take elevations contemporaneously with his, and notice and describe the appearances of the aurora.

It may be best to give a brief account of the successive appearances of the arch, and of those of the contemporaneous phenomena, before presenting the measures of its elevations. When first seen, its lower edge was not yet above the northern hills; but at half-past six o'clock it was quite clear of the horizon at its vertex, and at that point was about 9° broad. The light at this time was nebulous, and equally clear in all its parts, with no certain display of streamers. The upper edge vanished off from the brightest light within the limits of rather less than a degree. At the lower edge the vanishing off was considerably broader. At $6^h\ 35^m$ P.M. a brilliant display of streamers suddenly appeared over Callievar, a detached mountain three miles due west of the Manse, somewhat higher than any part of the Coreen hills; and over that mountain a solitary dense cloud was formed, two or three minutes after the appearance of the streamers there. These phenomena were quite disjoined from the arch, which in the mean time rose gradually higher above the horizon, and in place of the nebulous light began to exhibit, especially near the vertex, streamers or pencils of rays pointing upwards. These were not very brilliant, but quite defined. At $6^h\ 40^m$, a few detached very brilliant streamers, whose lower ends were concealed by the hills, appeared under the arch a little to the eastward of its vertex. These seemed to flit with great velocity from west to east, and from east to west, and appeared and disappeared in quick succession for about the space of five minutes. Their upper ends passed the lower edge of the arch, but never extended so high as the upper edge. When they disappeared the lower edge presented the same regularity and gradual vanishing off of the light as before their appearance. At 7^h P.M. the play of streamers across the arch had become somewhat more brilliant; and its edges remained equally even and well-defined as at the earlier periods, the upper one still maintaining its superiority over the lower in that respect. The arch was at this time about 12° broad; and a few very small clouds appeared in its west end, concealing the light at their places, but the edges were clear. At $7^h\ 5^m$, although the clouds had increased in number, they formed no impediment to the determination of the elevations; but by $7^h\ 10^m$ the whole sky had become too much obscured to admit of longer continued contemporaneous observations. At $7^h\ 25^m$ an opening in the clouds permitted the vertex of the arch to be seen, at the Manse, when it was observed to be still regular in its edges, and to have become of higher elevation.

Observed Angular Elevations of the vertex at the upper edge of an arch of aurora borealis, by the Rev. **James Farquharson**, at Manse of Alford, and contemporaneously by the Rev. **Hugh M'Connach**, at Hillhead of Kingsford, on the same magnetic meridian with Manse of Alford, distant from it in a southerly direction 6810 feet, and about 300 feet higher in level.

	At Manse of Alford. Angle of elevation.	At Hillhead of Kingsford. Angle of elevation.
At $6^h\ 45^m$ P.M.	16°	
6 50	17½	
6 55	19	
*7 0	21	14°
7 5	22	15
7 10	arch obscured by clouds at both stations.	
7 25	28°	seen only at Manse of Alford.

In making out the calculations of the height from these data, it is obvious that we may disregard the corrections for the rotundity of the earth, the difference of refraction at the two stations, and the difference of the horizontal and inclined distances of the stations, as they are all within the limits of the effect of the probable errors in observing a somewhat undefined object like the edge of the arch. But there is a considerable correction to be made in the angle observed at the Hillhead of Kingsford station, arising from its height above the level of Manse of Alford. Several years ago I had occasion to determine this, and found it about 300 feet by the barometer. This correction and the mode of calculation will become evident by means of the following diagram, which represents the first contemporaneous observations at seven o'clock.



Calculating from these data, we have the height D E = 5693 feet.

Thus the height of the upper edge of the aurora was 5693 feet above the level of the Manse of Alford. The vertex of the arch is found from these observations to have been 14831 feet northward of the same place, that is, over the nearest summits of the Coreen hills, two of which, it has been stated, are respectively 12500 and 15700 feet distant in the same direction. The most continuously elevated ridge of the same hills is about two miles farther north, and it was probably over it that the arch had its commencement.

At the time of the contemporaneous observations, whose results are given, the arch was 12° broad. This gives by calculation 3212 feet for the vertical extension of the fringe of streamers, and leaves 2481 feet for the height of the lower edge above the level of Alford. The tops of the Coreen hills are about 1000 feet higher than that level; and thus the lower edge of the arch, whose elevation was measured, was only about 1500 feet above these hills at their highest points.

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* Mr. M'Connach did not reach Hillhead of Kingsford to get observations before seven o'clock.

The result of this measurement agrees sufficiently with that of incidentally contemporaneous observations made by the Rev. JAMES PAULL of Tullynessle and me, on the 20th of December, 1829, as reported by me in a communication to the Royal Society, honoured with a place in the Philosophical Transactions of 1830*, and by which it appeared the aurora of that evening was within the limits of 4000 feet above the earth. These observations, although not made with instruments, were too decisive in all their conditions of the length of base line, clearness of parallax, and certainty that we had both seen the same phenomenon, to admit of doubt of the very limited elevation above the earth. The result agrees also with the ingenious observation of Mr. MINTO, and with that which might have been considered decisive of the question, namely, the observation of Captain PARRY and Lieutenants SHERER and ROSS, on the 27th of January, 1825, of the light of the aurora coming between them and a neighbouring height.

JAMES FARQUHARSON.

Alford, May 13, 1839.

P.S. I delayed making this Report, in expectation of finding other opportunities of making similar measurements, with the view of determining the variations of height to which the aurora is liable. My former communications upon the subject went to show that the height is dependent on that of the clouds, an inference which it will be observed is strengthened by some things contained in this Report. There have been many displays of aurora since the 20th of December last, but none that I have seen having the necessary conditions for obtaining a just measurement of the height; and as the ordinary season of its appearance is now over, I do not longer withhold the Report.

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* pp. 104, 105, 106.

XVII. *On the Constitution of the Resins. Part II.* *By* James F. W. Johnston, *Esq. M.A., F.R.S., Professor of Chemistry and Mineralogy in the University of Durham.*

Received May 9,—Read June 20, 1839.

III. *The Resin of Gamboge, or Gummi Gutt.*

THE medical and commercial values of the several varieties of gamboge which occur as articles of commerce in the European markets, and their botanical origin, have lately been investigated by Dr. Christison*. He found the different varieties to contain respectively

	Siam.		Ceylon.	
	Pipe.	Cake.	1.	2.
Resin	72·2	64·8	70·2	75·5
Cerabin	23·0	20·2	19·6	18·3
Cerasin	......	......	......	0·7
Fecula	......	5·6	......	......
Woody fibre, &c.	......	5·3	5·6	......
Moisture......	4·8	4·1	4·6	4·8
	100	100	100	99·3

The resin of gamboge is soluble in alcohol, and largely in ether; and so far as experiments go may be obtained by either solvent free from the foreign substances with which it is mixed in the resin of commerce. Like many of the other resins, acid as well as basic, it retains with considerable obstinacy the last traces of these solvents, and requires the prolonged action of a temperature above that of complete fusion fully to expel them.

1. 5·32 grains of the resin precipitated by water from its solution in alcohol, dried and fused at 212° Fahr., gave $\ddot{C} = 13{\cdot}98$, and $\dot{H} = 3{\cdot}52$ grains.

2. 6·685 grains of the same gave $\ddot{C} = 17{\cdot}64$, and $\dot{H} = 4{\cdot}43$ grains.

3. 7·675 grains of the resin obtained by evaporating the ethereal solution and prolonged heating, gave $\ddot{C} = 20{\cdot}060$, and $H = 5{\cdot}04$ grains.

4. 8·235 grains of the same heated to 350° Fahr., gave $\ddot{C} = 21{\cdot}640$, and $\dot{H} = 5{\cdot}365$ grains.

5. To be certain that alcohol and ether were absent, the resin was dissolved in caustic potash, precipitated by muriatic acid, washed, dried, and fused at 350° Fahr. In this state it fused less readily, and was of a darker colour.

13·146 grains gave $\ddot{C} = 34{\cdot}432$, and $\dot{H} = 8{\cdot}67$ grains.

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* Proceedings of the Royal Society of Edinburgh, March 7, 1836.

These results give per cent. respectively,

	From alcohol.		From ether.		After solution in potash and fusion at 350°.
	Fused at 212°.	Fused at 212°.	Fused at 212°.	Fused at 350°.	
Carbon ..	72·662	72·763	72·280	72·668	72·406
Hydrogen	7·352	7·363	7·296	7·225	7·243
Oxygen ..	19·986	19·874	20·424	20·351	20·351
	100	100	100	100	100

It is difficult to arrive at a satisfactory conclusion in regard to the true formula for this acid resin.

The results of analysis approach exceedingly near to the beautifully simple formula $C_5\,H_3\,O$, or $C_{40}\,H_{24}\,O_8$, which gives

		Calculated.	Experiment.
$C_5 =$	$382{\cdot}185 =$	$73{\cdot}550$	$72{\cdot}763$
$H_3 =$	$37{\cdot}438 =$	$7{\cdot}205$	$7{\cdot}363$
$O_1 =$	$100{\cdot}000 =$	$19{\cdot}245$	$19{\cdot}874$
	$519{\cdot}623$	100	100

This formula is further interesting, because of the close relation it would establish between oil of turpentine and its resin, and that of gamboge; since

Oil of turpentine.

$$C_5\,H_4 \left\{ \begin{array}{l} -\,H \\ +\,O \end{array} \right. = C_5\,H_3\,O.$$

The fact, however, that in all the analyses there is a constant deficiency of carbon to the amount of nearly one per cent., renders the formula suspicious. It is true that the gambodic acid has a strong affinity or attraction for ether and alcohol, and that a certain quantity of these substances may have been present in some of the specimens analysed; yet it would be necessary to suppose a much larger admixture of them than seems possible, in order to account for so great a diminution in the per centage of carbon. Neither can we admit the presence of these substances in the resin after solution in potash, though the carbon in this also is considerably less than the formula requires.

In order to determine whether a still higher temperature than 350° Fahr. would drive off a further portion of alcohol from the resin precipitated from its solution by water, I heated it to 400° + Fahr., when it ceased to emit its agreeable fragrant odour, gave off white vapours, and became slightly darker in colour. In this state 11·38 grains gave $\ddot{C} = 30{\cdot}23$, $\dot{H} = 6{\cdot}73$ grains, or per cent.,

$$\begin{array}{lcr} \text{Carbon} & = & 73{\cdot}791 \\ \text{Hydrogen} & = & 6{\cdot}601 \\ \text{Oxygen} & = & 21{\cdot}608 \\ \hline & & 100 \end{array}$$

The carbon here is sufficiently great, but there is much too little hydrogen for the formula ($C_5\,H_3\,O$), to which this resin seems so closely to approximate.

After fusion at this temperature, however, the gambodic acid was no longer wholly soluble in alcohol, nearly one half of its bulk being left behind in the form of a yellow powder when digested in this liquid. This powder was collected, washed with alcohol, and heated for a long time at 212° + Fahr. previous to analysis. At 400° Fahr. it undergoes no apparent change; at 500° Fahr. it becomes brown, and gradually darkens, but does not melt. 7·30 grains gave $\ddot{C} = 18{\cdot}93$, and $\dot{H} = 4{\cdot}62$, or per cent.,

	Experiment.	Calculated.
Carbon	= 71·703	72·033
Hydrogen	= 7·031	6·762
Oxygen	= 21·266	21·205
	100	100

This result agrees with the formula $C_{40}\,H_{22}\,O_9$, according to which the second column is calculated.

The formula which most nearly represents the result of experiments on the unchanged resin is $C_{48}\,H_{29}\,O_{10}$. This gives

		per cent.
C_{48} =	3668·976 =	72·929
H_{29} =	361·908 =	7·193
O_{10} =	1000·000 =	19·878
	5030·884	100·000

in which there is an allowance for loss of carbon and for a similar slight excess of hydrogen in the results of analysis, being the directions in which errors are likely to occur.

It is satisfactorily ascertained, however, that the gamboge of commerce, as we have found reason to conclude is the case with dragon's blood, undergoes a process of manufacture before it is brought to market. Like the reed dragon's blood, it has all the appearance of having been fused and moulded into shapes; the effect of such fusion in the case of the red resin is, as we have seen*, to diminish the amount of carbon about one per cent., and if we suppose a similar change produced on the gamboge, the deficiency of carbon in the analysis will be sufficiently accounted for. The likelihood of an analogous change in the two resins is the greater, as they belong apparently to a kindred group, the several members of which are related almost as closely as mastic and colophony. On comparing the quantity of hydrogen found by analysis with that given by the formula $C_{40}\,H_{24}\,O_8$, 7·205 per cent., we find them approach so closely as scarcely to allow for the necessary errors of experiment, but in the reed dragon's blood there was a similar deficiency, so that until we have the opportunity of analysing the gamboge resin as it issues from the tree, we might adopt the formula to which the results appear to point. There is an analogy, however, in favour of the formula

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* See page 134 of the present volume.

$C_{40} H_{23} O_8$ containing one equivalent less of hydrogen, which is not unworthy of attention. When heated above 400° FAHR. we have seen that this resin is partly changed into a nearly insoluble resin $= C_{40} H_{22} O_9$, that is

$$C_{40} H_{23} O_8 \left\{ \begin{matrix} -H \\ +O \end{matrix} \right. .$$

In like manner mastic resin when so heated is in part changed into $C_{40} H_{30} O_5$, that is

$$C_{40} H_{31} O_4 \left\{ \begin{matrix} -H \\ +O \end{matrix} \right. .$$

It is reasonable to suppose that the nature of the change which takes place is the same in both; and though I do not consider, notwithstanding the precautions adopted, that the number of atoms of hydrogen in any of the formulæ I have given for the resins can be considered *absolutely certain*, yet there being a reasonable probability that mastic resin A, is represented by $C_{40} H_{31} O_4$, there is reason from the present analogy to infer that gamboge resin is $C_{40} H_{23} O_8$.

One interesting and important fact may be considered as established, whichever of these formulæ be adopted, that a small change in the number of atoms of hydrogen only, may give birth to new resins possessed of very different properties. We have for example,

Colophony $= C_{40} H_{30} O_4$ Liebig.
Mastic A. $= C_{40} H_{31} O_4$

represented generally by

$$C_{40} H_{32-x} O_4$$

the resin of which

$C_{40} H_{32} O_4$ is the formula, being undetermined.

And we have also

Resin of dragon's blood $= C_{40} H_{21} O_8$
Resin of gamboge. . . . $= C_{40} H_{23} O_8$?

And the general formula

$$C_{40} H_{24-x} O_8$$

may represent a second large group, of which, as in the former, only two members are as yet determined.

In the present stage of our inquiry it would be premature to dwell on theoretical indications as to what we may hereafter expect to meet with, but it may be remarked that H_{32} and H_{24} seem to point to a series, of which H_{64}, H_{16} and H_8 are members, and which may respectively form the starting-points as it were, of new groups.

Salts of Gambodic Acid.

Gambodiates of Potash and Soda.—The salts of the alkaline metals may be formed by dissolving or digesting the acid in solutions of the caustic alkalies. In strong solutions the salt is very sparingly soluble, so that by digesting the acid in such solution, the compounds may easily be obtained in a solid state. In colour they are somewhat darker than the pure resin, and very soluble in pure water, giving reddish-brown solutions, and though more sparingly, yet still largely soluble in alcohol. They are decomposed by the dilute mineral acids, which precipitate the acid from its saline solutions unchanged, and in the form of a pale-yellow bulky powder.

Gambodiate of Ammonia.—Gambodic acid dissolves slowly in dilute caustic ammonia, giving a brownish-red solution. The solution is most readily obtained by adding caustic ammonia to the alcoholic solution of the acid, diluting largely with water and distilling or boiling off the alcohol and excess of ammonia. Evaporated to dryness at a temperature below 212° FAHR., it leaves a brittle resinous mass having the colour of the acid. Instead of the fragrant smell of the resin, however, it has assumed a faint unpleasant animal odour. It is insoluble in pure water, but dissolves on the addition of a little caustic ammonia. It dissolves also in alcohol, and is decomposed by the mineral acids. Boiled in a solution of caustic potash, ammonia is evolved and a salt of potash formed. Its solution in water precipitates the ammonia nitrate of silver and the ammonia acetate of lead (PAYEN's salt) of a yellow colour, and the ammonia sulphate of copper of a dark-reddish-brown.

Earthy and Metallic Salts.—On these salts I have made many experiments with results in some cases sufficiently discordant, and which led me at first to consider that the equivalent of this resin should be represented by ($C_{50}H_{29}O_{10}$), instead of ($C_{40}H_{23}O_8$), as the analysis of the resin appears to indicate. I am now inclined, however, to adopt the formula containing C_{40}, as entering into the expression for the gambodiates, and to attribute the observed differences between experiment and calculation to the difficulty of obtaining these as well as other resinous compounds in a perfectly pure state, especially free from mutual admixture. To this conclusion I am further led by the study of the salts of mastic resin A, of which an account has already been given.

Properties of the Earthy and Metallic Gambodiates.—1. When fresh prepared from alcoholic solutions these salts are yellow, slightly soluble in alcohol, and insoluble in water. When first precipitated from solutions of chloride of calcium and nitrate of strontia, by means of ammonia, they are of a beautiful red, but by standing they subside yellow. Precipitated from ammoniacal *aqueous* solutions, they are brownish or brownish-red.

2. Collected on the filter after precipitation from alcoholic solutions, washed with alcohol, and dried by pressure on bibulous paper, they are in the form of a beautiful yellow powder, in which state they remain, if dried by a very gentle heat in the open air, raised after a sufficient time to 212° FAHR. If immediately on being dried by pressure as much as possible between folds of paper, they are exposed to a heat of upwards of 100° FAHR., they melt, and run into a thin liquid, which on cooling solidifies into a red brittle resin, giving a yellow powder. This property, which is common to these with the salts of many other resins, appears to indicate a combination of the salts with alcohol, from the last portions of which, like the resins themselves, they

are with difficulty separated. They may be heated, however, to 350° or 400° Fahr., without decomposition, emitting only the fragrant odour characteristic of the gambodic acid itself. At or under this temperature they melt, those of the earths excepted, and on cooling assume the appearance of a reddish-brown resin, not unlike the acid, and like it giving a beautiful yellow powder. At a higher temperature they are decomposed, giving off empyreumatic products. The salt of silver is discoloured by exposure to the air, and when dry is of a dirty-dark-green. They are all decomposed by boiling in acetic and dilute nitric acids.

I had spent much time in preparing these salts by means of alcoholic solutions, before I discovered that the resin possesses a property which enables us to prepare very many of its salts from aqueous solutions. The gambodiate of ammonia being soluble in dilute ammonia, and *not* precipitable by water, it is sufficient to add caustic ammonia to an alcoholic solution of the resin, and to dilute with water, when the addition of an ammoniacal solution of magnesia, oxide of manganese, oxide of copper, oxide of zinc, acetate of lead, or of any other oxide soluble in caustic ammonia, causes a precipitate of the corresponding gambodiate of a brownish-red colour. By washing with dilute ammonia these may be obtained in a state of comparative purity*. Analogous compounds may be formed by adding an alcoholic solution of the resin to similar solutions of the metallic salts with or without the addition of ammonia.

A. *Salts of Lead.*—1. When a solution of the resin in alcohol is mixed with one of acetate of lead, a yellow precipitate falls. Washed with alcohol and dried

				per cent.
a. At	212° Fahr.,	11·425 grs. gave	2·224 oxide of lead	= 19·466
b. After fusion, at	370° Fahr.,	13·03 grs. gave	2·58 oxide of lead	= 19·80

A second portion thrown down from a solution in which a large excess of resin was present gave only 17·36 per cent. of oxide. A *sesquisalt* $= 3\ (C_{40}\ H_{23}\ O_8) + 2\ Pb\ O$, contains 18·32 per cent. of oxide, and it will be recollected that mastic resin A. in similar circumstances gave also a sesquisalt†.

2. To the mixed solutions as above, after the precipitate was separated, caustic ammonia was added with agitation. The new precipitate was also yellow, but when washed and dried 14·74 grs. gave only 2·046, or 13·88 per cent. of oxide of lead. This agrees with the formula $Pb\ O + 2\ (C_{40}\ H_{23}\ O_8)$ which contains 14·40 per cent. of oxide, though it is not easy to understand why a *bi*salt should be thrown down when the solution is rendered more neutral by the addition of ammonia.

3. An ammoniacal solution of the resin was diluted with water, and a solution of Payen's ammoniacal subacetate of lead was added to it. A yellow precipitate fell which was collected, carefully washed, and dried at 212° Fahr. At a low heat it took fire and burned like tinder, without melting.

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* This property renders gamboge of great value for experimental illustration of the properties of the resins in the Class-room.

† Vid. ante, p. 124.

10·733 grs. left 5·284 grs. of oxide of lead = 49·231 per cent. This indicates three of base to one of acid; the formula $3\ PbO + (C_{40}\ H_{23}\ O_8)$ requiring 50·23 per cent.; and this tribasic salt is precisely the analogue of the *tribasic* acetate from which it is formed.

After being digested in acetic acid, and again washed and dried, this salt lost five-sixths of its base, becoming a *bi*salt. Thus 11·066 grains left 1·635 of oxide of lead = 14·777 per cent.; the formula, as already stated, requiring 14·40 per cent. That an acid salt should be formed under these circumstances is what we should expect; but whether a *bi*salt is the constant result I have not determined.

B. *Salts of Copper.*—1. A dilute ammoniacal solution of the resin in water was mixed with one of ammoniaco-sulphate of copper; a brownish-yellow precipitate fell, which was washed with dilute ammonia, and dried at 212° FAHR.

11·037 burned in the air left 1·197 $\dot{Cu}$ = 10·845 per cent.
13·455 of another portion left 1·40 $\dot{Cu}$ = 10·405 per cent.

A portion of the dry salt, after boiling in dilute ammonia, still left 10·994 per cent. of oxide.

These results indicate a neutral salt, the formula $\dot{Cu} + (C_{40}\ H_{23}\ O_8)$ requiring 10·68 per cent. of oxide of copper*.

2. A further portion of the salt was prepared by adding the ammoniacal resin to a solution of common sulphate of copper; and, as this might contain excess of oxide, one portion of it was digested while moist in *hot* dilute ammonia. When dried it left in two experiments 18·951 and 18·830 of oxide per cent. respectively. Another portion was boiled in strong caustic ammonia, by which, as appeared from the colour of the solution, a portion of the acid was taken up. Washed, dried, and burned, it left 14·666 per cent. of oxide of copper.

The former result agrees with $2\ \dot{Cu} + (C_{40}\ H_{23}\ O_8)$, which requires 19·31 per cent. of oxide.

The latter agrees with $3\ \dot{Cu} + 2\ (C_{40}\ H_{23}\ O_8)$, which requires 15·21 per cent. of oxide.

It is impossible, however, without several repetitions of these experiments, to say that these coincidences are more than accidental, though from analogy it appears probable that such compounds do exist. Above all, their true nature can only be known by the ultimate analysis of the salts,—a work of great labour, but which I do not despair of being able to undertake. By varying the mode of preparation I obtained a copper salt by direct precipitation, containing 14·824 per cent. of oxide, agreeing with one of those just described. But if the three classes of salts above obtained be constant and definite, the circumstances under which they may be formed without failure requires further study.

C. *Salts of Zinc.*—The gambodiate of zinc is obtained by mixing the ammoniacal resin with a solution of zinc in ammonia, or with an ammoniacal solution of sulphate of zinc. It is of a brownish-yellow colour. If the solution be too largely diluted with water, an excess of oxide of zinc is apt to be thrown down along with the salt. Thus the first portion I prepared gave me 14·17 per cent. of oxide of zinc, while another prepared with more care gave me in two experiments 11·242 and 10·976 per cent. respectively. A neutral salt $= Zn\,O + (C_{40}\,H_{23}\,O_8)$, contains 10·83 per cent. oxide of zinc.

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* The equivalent of oxide of copper being 495·695.

D. *Salts of Lime, Strontia, and Magnesia.*—*a.* To alcoholic solutions of chloride of calcium and of nitrate of strontia, ammonia was added, and afterwards a solution of gambodic acid in alcohol. The former gave an abundant, the latter a very sparing precipitate. The mixed solutions at first became of a beautiful red colour, but the precipitate, as it fell, gradually assumed a more yellow shade. Washed without access of air, and dried at 360°, the lime salt gave in two experiments 6·119 and 6·192 per cent. of lime; the strontia salt gave 9·877 per cent. of strontia.

If these results are to be depended upon, they indicate a *sesqui*salt in both cases; since

$\dot{Ca} + 1\frac{1}{2}\,(C_{40}\,H_{23}\,O_8)$ contains 5·72 per cent. of lime,

and

$\dot{Sr} + 1\frac{1}{2}\,(C_{40}\,H_{23}\,O_8)$ contains 9·43 per cent. of strontia.

b. The ammoniacal resin in water gives, with ammoniacal sulphate of magnesia, a copious precipitate, which, dried and burned, yielded 4·542 and 4·681 per cent. of magnesia, which, however, is not reconcileable to any formula.

E. *Salts of Silver.*—1. Newly precipitated oxide of silver, digested in gambodic acid in alcohol, gave a dark-brown salt, which fused at 360° FAHR; and of which, when burned, 20·335 grains left 5·46 of silver = 28·762 per cent. of oxide. A neutral salt, $Ag\,O + (C_{40}\,H_{23}\,O_8)$, contains 25·94 per cent., to which the salt above obtained approximates. It will be readily understood that very careful agitation would be required to bring all the particles of the oxide introduced in the state of powder into contact with the resin held in solution, and that an excess of silver is to be expected in a salt prepared by such a process.

2. When an alcoholic solution of the resin is poured into a similar solution of nitrate of silver, no precipitate falls; but if the mixture be allowed to stand for some time, the inside of the glass vessel becomes covered with a coating of metallic silver, and a small quantity of a black powder gradually falls to the bottom. The same effect follows if the resin of dragon's blood be used, and in a still more marked manner with the pure resin of guaiacum. If ammonia be added, a yellow precipitate falls, which is speedily discoloured by the air, and ultimately becomes dark-green. Carefully dried,

6·94 grains left 1·00 grain of silver = 15·475 per cent. of oxide of silver.
7·766 grains left 1·146 grains of silver = 15·848 per cent. of oxide of silver.

These results indicate the formation, as was the case with mastic resin A*, of a *bi*-salt, containing also, like the salt of that resin, a small excess of silver. The formula $Ag\,O + 2\,(C_{40}\,H_{23}\,O_8)$ requires 14·90 per cent. of oxide of silver.

3. Into an aqueous solution of ammonia nitrate of silver, an ammoniacal solution of gambodic acid was slowly poured with agitation, a yellow precipitate began to fall, which re-dissolved on a further addition of the resin, and was again thrown down on adding ammoniacal nitrate of silver. The yellow precipitate gradually collected into brown flocks, and formed a brown coherent sediment, leaving the supernatant liquid of a pale yellow colour. The precipitate was well washed with distilled water, collected on the filter, and dried at 250° FAHR., till at that temperature it ceased to soften.

13·17 grains burned in the air left 1·17 of metallic silver, or 9·54 per cent. of oxide.

7·866 grains left 0·725 of silver = 9·90 per cent. of oxide.

A salt consisting of three equivalents of acid to one of base, or $Ag\,O + 3\,(C_{40}\,H_{23}\,O_8)$, contains 10·22 of oxide of silver. We may safely consider the compound prepared by this method, therefore, as a *ter*salt, the excess of ammonia present probably retaining the silver in solution, so as to prevent its forming a more basic combination. It is not unlikely that by varying the process a *bi*salt might be obtained from aqueous solutions similar to that which was given by alcoholic solutions.

Constitution of the Salts of Gambodic Acid.

Of the salts above described I have subjected to ultimate analysis only the *bi*salt of silver, obtained by mixing the alcoholic solutions of the resin and of nitrate of silver, and adding ammonia. This salt contained 15·88 per cent. of oxide of silver, and when burned with oxide of copper,

8·19 grains gave $\ddot{C} = 18{\cdot}02$, and $\dot{H} = 4{\cdot}47$.
7·60 grains gave $\ddot{C} = 16{\cdot}875$, and $\dot{H} = 4{\cdot}11$.

These results are equal to

	A.	B.	Calculated.
Carbon	= 60·954	= 61·40	61·51
Hydrogen	= 6·065	= 6·00	5·77
Oxygen	= 17·098	= 16·72	17·82
Oxide of silver	= 15·883	= 15·88	14·90
	100	100	100

The numbers in the third column are calculated according to the formula $Ag\,O + 2\,(C_{40}\,H_{23}\,O_8)$, and to these numbers the experimental results approximate very closely. Like the similar analyses of the salts of mastic resin A†, these show that in combining with oxides the gambodic acid does not part with the elements of water. They are opposed, however, to the view suggested by the constitution of some of the salts of that resin, that the metal replaces an equal number of equivalents of hydrogen when it combines with the resin; and they support rather the other view, which would represent the normal state of the acid resin to be that in which it exists when in a state of combination, and that the hydrogen contained in their salts of the metallic oxides is the true quantity which enters into the composition of these bodies.

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* Ante, p. 126.

† Ante, p. 128.

Conclusions.—From the preceding investigation it appears,

1. That the most probable formula for gamboge resin is $C_{40}\,H_{23}\,O_8$, though all the specimens analysed yield too little carbon by nearly one per cent. This deficiency is supposed to be due to a change produced during the preparation of the natural resin for the market.

2. When heated to about 400° Fahr. it undergoes partial decomposition, a resin soluble in cold alcohol being formed, and another insoluble in that liquid. The constitution of the latter seems to be represented by $C_{40}\,H_{22}\,O_9$.

3. It forms with the metallic oxides numerous classes of salts, the existence and constitution of which the preceding experiments must be considered as only rendering *probable*. A complete study of the resinous salts, and of the circumstances under which the several classes of them are necessarily formed, though it would involve much labour, would probably lead to interesting results. The various saline compounds obtained as above described may be classed as follows, representing the formula $C_{40}\,H_{23}\,O_8$ by $\overset{r}{G}$.

	Formula.	Oxide per cent.		How obtained.
		Found.	Calculated.	
1. *Tersalts* ...	$Ag\,O + 3\,\overset{r}{G}$	9·90	10·22	By mixing ammoniacal aqueous solutions.
2. *Bisalts* ...	$Ag\,O$	15·47	14·90	Mixing alcoholic solutions and adding ammonia.
	$Pb\,O\ \} + 2\,\overset{r}{G}$	14·77	14·40	By digesting in dilute acetic acid, the salt precipitated on mixing ammoniacal aqueous solutions.
3. *Sesquisalts*	$2\,Pb\,O$	19·46	18·32	Mixing the resin in alcohol with acetate of lead in alcohol.
	$2\,Ca\,O\ \} + 3\,\overset{r}{G}$	6·119	5·72	Mixing the resin with chloride of calcium in alcohol, and adding ammonia.
	$2\,Sr\,O$	9·87	9·43	Mixing the resin with nitrate of strontia in alcohol, and adding ammonia.
4. *Neutral* ...	$Ag\,O$	28·76	25·94	Digesting the resin on fresh precipitated oxide of silver.
	$Cu\,O\ \} + \overset{r}{G}$	10·84	10·68	From ammoniacal sulphate of copper by resin in dilute ammonia.
	$Zn\,O$	10·97	10·83	From ammoniacal sulphate of zinc by resin in dilute ammonia.
5. *Subsesqui*	$3\,Cu\,O + 2\,\overset{r}{G}$	14·66	15·21	Boiling the fresh precipitated $Cu\,O + \overset{r}{G}$ in concentrated caustic ammonia.
6. *Disalts* ...	$2\,Cu\,O + \overset{r}{G}$	18·95	19·31	Digesting do. in hot dilute caustic ammonia.
7. *Tribasic* ...	$3\,Pb\,O + \overset{r}{G}$	49·23	50·23	Mixing ammonia acetate of lead (tribasic) with the resin in dilute caustic ammonia.

I cannot persuade myself to regard this formidable list of salts without suspicion, especially as several of them have been formed only once. In detailing my results and observations, however, I have in some measure cleared the way for future inquirers.

IV. *Resin of Guaiacum.*

When the resin of guaiacum of the shops is treated with alcohol in the cold it is nearly all dissolved, giving a dark-brown solution. On evaporating this solution in a flat dish, and heating it for twelve hours at a temperature rising from 180° to about 250° Fahr., a very beautiful transparent ruby-coloured resin is obtained, brittle and electric, melting at 212° Fahr., and emitting an agreeable fragrant odour.

Of this resin thus carefully dried,

A. 10·647 grs. gave $\ddot{C} = 27{\cdot}167$ and $\dot{H} = 6{\cdot}585$
B. 10·995 grs. gave $\ddot{C} = 27{\cdot}975$ and $\dot{H} = 6{\cdot}733$

these results gave per cent.

	A.	B.
Carbon . =	70·555	70·35
Hydrogen =	6·870	6·80
Oxygen . =	22·575	22·85
	100	100

and they correspond most closely with the formula $C_{40}\,H_{23}\,O_{10}$ which gives

40 Carbon . = 305·75 = 70·37
23 Hydrogen = 28·70 = 6·60
10 Oxygen . = 100·00 = 23·02
434·45 100

This resin in a natural arrangement, therefore, must stand in the same group with the resins of dragon's blood and of gamboge, which as we have already seen may be represented by

Resin of dragon's blood $C_{40}\,H_{21}\,O_{8}$
Resin of gamboge . . . $C_{40}\,H_{23}\,O_{8}$
Resin of guaiacum . . $C_{40}\,H_{23}\,O_{10}$

Salts of Guaiacum Resin.—When the solution of this resin in alcohol is added to an alcoholic solution of acetate of lead in excess, a white precipitate falls, which becomes blue on exposure to the sun's rays[*]. After separating the precipitate, ammonia throws down a further portion from the supernatant liquid. I have analysed three successive portions of the first of these compounds, which contains two equivalents of oxide of lead to forty of carbon; but the results are so little concordant with the analysis of the uncombined resin as to render further investigation necessary.

With nitrate of silver this resin gives no precipitate, but on standing exposed to the light, the silver, in considerable quantity, is thrown down in the metallic state. I have not examined what change of composition the resin undergoes during this deoxidation of the metal.

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[*] A delicate photogenic paper may be formed by first washing with an alcoholic solution of guaiacum resin, and afterwards with one of neutral acetate of lead.

V. *Acaroid Resin.* (*Botany Bay Resin, Yellow Gum.*)

This resin exudes from the *Xanthorrhœa hastilis,* is of a darker reddish yellow colour than gamboge, comes to this country generally mixed with the spines and bark of the tree, gives in alcohol and ether a much darker solution than gamboge, and a darker resin when evaporated. It emits also a peculiar fragrant odour when melted, and differs further from gamboge in being almost entirely precipitated by water from its solution in alcohol, even when much ammonia has been added. Dried in a thin film at 250° Fahr. for twelve hours,

A. 11·655 grs. gave $\ddot{C} = 28·523$ and $\dot{H} = 6·036$
B. 11·61 grs. gave $\dot{H} = 6·048$

and of another portion similarly heated

C. 10·236 grs. gave $\ddot{C} = 25·204$ and $\dot{H} = 5·258$

	A.	B.	C.
Carbon . =	67·67		68·085
Hydrogen =	5·75	5·78	5·707
Oxygen . =	26·58		26·208
	100		100

And they indicate the formula $C_{40} H_{20} O_{12}$ which by calculation gives

40 Carbon . =	3057·480 =	67·84
20 Hydrogen =	249·592 =	5·54
12 Oxygen =	1200·000 =	26·62
		100

The slight excess of carbon in analysis C is no doubt an error of experiment.

This resin, therefore, belongs also to the group represented by the general expression $C_{40} H_{24-x} Oy$, and it contains more oxygen than any other resinous substance hitherto analysed.

General Remark.—In so far as we are entitled to draw general conclusions from the analyses contained in this and the preceding paper, it appears,

1. That *many* of the resins may be represented by formulæ exhibiting their elementary constitution, and the weight of their equivalents in which 40 C is a constant quantity.

2. That there appear to be groups in which the equivalents both of the carbon and the hydrogen are constant, the oxygen only varying, and others in which the hydrogen alone varies, the two other elements being constant.

In a subsequent paper I hope to extend further these first generalizations.

Durham, May 2, 1839.

XVIII. *On the Constitution of the Resins. Part III.* By JAMES F. W. JOHNSTON, *Esq. M.A., F.R.S., Professor of Chemistry and Mineralogy in the University of Durham.*

Received June 11,—Read June 20, 1839.

VI. *Resin of Sandarach.*

THE resin of sandarach dissolves readily in alcohol, giving a yellow solution, which may be obtained of the consistence of a balsam. It dissolves without appreciable residue.

If this solution be largely diluted with alcohol, a white flocky resin separates in small quantity, falls slowly to the bottom, and may be procured in a state of considerable purity by decantation and washing with cold alcohol.

If to the alcoholic solution solid caustic potash, or a concentrated solution of this alkali in water be added, a precipitate falls, which re-dissolves if the potash be present in great excess. By adding, to a solution containing potash in excess, a solution of the resin as long as any precipitate falls, the whole of the resin thus separable may be thrown down. It may be washed by affusions of hot alcohol, by which a portion of it is dissolved; and the alcohol it holds apparently in combination, may be separated by heating for a sufficient length of time at a temperature not exceeding 250° FAHR.

I. *Resin* A. *of Sandarach.*—The white flocks which fall to the bottom of the solution of sandarach resin in alcohol, being collected on the filter, washed with alcohol, and afterwards boiled in water, are obtained in the form of a white powder, having neither taste nor smell. Dried at 212° FAHR. this resin undergoes no change; but if the temperature be raised to about 300° FAHR., it gradually assumes a yellow colour, and slightly coheres, but does not melt. If the heat be still increased, the colour becomes brown, apparently indicating incipient decomposition.

1. Of a portion of the resin thus obtained, and heated till it became yellowish,

A. 11·175 grains gave $\ddot{C} = 31{\cdot}70$, $\dot{H} = 9{\cdot}86$.
B. 11·095 grains gave $\ddot{C} = 31{\cdot}315$, $\dot{H} = 9{\cdot}818$, or per cent.,

	A.	B.
Carbon	= 78·437	78·043
Hydrogen	= 9·803	9·832
Oxygen	= 11·760	12·125
	100	100

2. Of a portion obtained by the same method from a second quantity of the crude resin, and well washed,

11·802 grains, heated till it acquired a tinge of yellow, gave $\ddot{C} = 33·06$, $\dot{H} = 10·53$, or, per cent.,

Carbon = 77·456
Hydrogen = 9·913
Oxygen = 12·631
100

This analysis, which only the loss of the remainder of my resin by an accident prevented me from repeating, agrees with the formula $C_{40}\ H_{31}\ O_5$, which gives

	Calculated.		By experiment.		
			First portion.		Second.
40 Carbon	= 3057·480	= 77·515	78·437	78·043	77·456
Hydrogen	= 386·867	= 9·808	9·803	9·832	9·913
Oxgen	= 500·000	= 12·677	11·760	12·125	12·631
	3944·347	100	100	100	100

There can be little doubt, therefore, that the sparingly soluble resin of sandarach contains five of oxygen to forty of carbon; and as more care was taken to purify by washing with alcohol the portion which gave the result in the last column (2.), the true formula is most probably that above given, $C_{40}\ H_{31}\ O_5$.

II. *Resin* B. *of Sandarach*.—The alcoholic solution from which caustic potash ceased to throw down any precipitate was decanted, and a portion of the alcohol separated by distillation. It was then largely diluted with water, by which it was only slightly troubled; the resin was precipitated by muriatic acid, collected on the filter, and washed by repeated affusions of hot water. After drying at 212° Fahr. this resin was *wholly dissolved by common alcohol in the cold.* According to Unverdorben the resin of sandarach consists of three resins, one thrown down from the alcoholic solution of the crude resin by caustic potash, the resin C. of Unverdorben and of the present paper, and two which remain in solution unaffected by the potash*. When these mixed resins are separated from the potash by muriatic acid, in a way similar to that above described, he states that boiling alcohol dissolves the one and leaves the other. In my experiments, as above detailed, cold alcohol and cold ether also dissolved the whole of what I supposed to contain two resins; while from the solution of the crude resin, by simple dilution a precipitate fell, not noticed by Unverdorben, and possessing characters different from those exhibited by any of the three resins described by that experimenter.

Evaporated and heated at 212° Fahr., as long as any apparent change was produced, the alcoholic solution gave a bright yellow resin, brittle when cold, but softening at the temperature of boiling water.

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* Thomson's Organic Chemistry, p. 529.

A. 10·407 grains of it in this state gave $\ddot{C} = 28·83$, and $\dot{H} = 9·40$.

As this does not give a formula containing C_{40}, I exposed the resin dried in mass at 212° Fahr. to a higher temperature, when it melted, and frothed up, giving off vapours. To obtain it free from water and any other volatile substances it might retain, I evaporated a second portion of the alcoholic solution, and kept the resin in the state of a thin film for forty-eight hours at 200° Fahr.

B. 10·515 grains now gave $\ddot{C} = 28·58$, and $\dot{H} = 9·295$.

Still further heated for twenty hours at 250° Fahr., the resin was yellow and brittle, and

C. 11·69 grains gave $\ddot{C} = 32·055$, and $\dot{H} = 10·22$.

These three results are equal respectively to

Dried in mass at 212°.	In a thin film.	
	At 200° for 48 hours.	Further 20 hours at 250° Fahr.
Carbon = 76·601 Hydrogen = 10·038 Oxygen = 13·361	75·08 9·82 15·10	75·82 9·71 14·47
100	100	100

The analyses B. and C. agree very closely with the formula $C_{40}\,H_{31}\,O_6$, which gives

Carbon = 75·59
Hydrogen = 9·56
Oxygen = 14·85
100

The slight excess of carbon in the third analysis, if not an error of experiment, indicates that the heating had been carried a little too far; while the similar excess both of carbon and hydrogen in the resin dried only at 212°, would imply the presence of a volatile carbo-hydrogen, which is wholly expelled only after long-continued heating. A familiar example of the presence of such a volatile compound, we have in the association of oil of turpentine with common resin; and the fact that most of the resins yield on distillation with water a volatile oil, gives ground for believing that the presence of substances analogous to oil of turpentine may tend more or less to modify or vitiate the results obtained by the analyses of resinous bodies, when sufficient precautions are not taken to ensure their expulsion.

III. *Resins precipitated by Caustic Potash.*—The precipitate thrown down by caustic potash from the alcoholic solution of the sandarach of commerce, dissolved readily in a hot dilute solution of caustic potash. From this solution largely diluted with water, the resin was precipitated by dilute muriatic acid, and afterwards washed on a filter till it ceased to render the water acid. Being then boiled in water, dried at 212°, and digested with boiling alcohol, only a small quantity was taken up. The solution on evaporation gave a pale-yellow resin which did not fuse at 250° Fahr., but cohered at 300° Fahr.

A. 10·817 grs. gave $\ddot{C} = 29{\cdot}875$, and $\dot{H} = 9{\cdot}59$.

As this *first* solution was likely to contain a portion of the more soluble resin B. carried down by the insoluble potash salt, when precipitated from the solution of the crude resin, the undissolved portion was boiled in a second and third quantity of alcohol. This third solution was evaporated, and the pale-yellow resin heated to 230° Fahr. for forty-eight hours.

B. 8·3 grs. gave $\ddot{C} = 23{\cdot}1$, and $\dot{H} = 7{\cdot}45$

These two analyses are equal to

	A.	B.
Carbon =	76·37	76·95
Hydrogen =	9·85	9·97
Oxygen =	13·78	13·08
	100	100

The result of the first analysis is intermediate between those obtained from resins A. and B., the second which certainly represents the constitution of a purer form of the resin, approaches very nearly to the constitution of resin A. represented by the formula $C_{40}\ H_{31}\ O_5$.

It would appear, therefore, that one resin A. is to a certain extent soluble in an alcoholic solution of B., but that in some of the varieties of sandarach the quantity of A. actually present is greater than can be thus taken up, in consequence of which a portion sometimes remains behind undissolved when the natural resin is digested in cold alcohol. On treating with caustic potash, however, resin A. is wholly, or more correctly, perhaps almost entirely thrown down along with another resin C. and a small adhering portion of B. On decomposing this potash salt, as above described, with muriatic acid, the mixed resins are obtained and may be separated by boiling alcohol. Resin A. and the small quantity of B. are dissolved, giving a brownish yellow solution, from which on cooling, A. is in a great measure precipitated. Resin C. remains behind of a brown colour. After boiling in distilled water it is obtained in the form of a grey friable mass, which becomes yellowish when heated for twenty-four hours at 250° Fahr., but does not melt. At 500° Fahr. it melts into a brown semifluid mass, and speedily decomposes.

IV. *Resin* C. *of Sandarach.*—The mixed resins obtained from the potash salt by muriatic acid were boiled in repeated portions of common alcohol as above described. The insoluble portion was, in the hot alcohol, soft, glutinous, and of a brown colour. It was boiled in water, by which it was freed from alcohol, becoming hard and friable. Dried at 250°, it was in the state of a yellow powder.

A. 7·044 grs. burned in the air left 0·114 grs. of a grey residue, chiefly carbonate of lime, or 1·61 per cent.

B. 8·97 grs. gave $\ddot{C} = 24{\cdot}405$, and $\dot{H} = 7{\cdot}614$ grs.
C. 9·08 grs. gave $\ddot{C} = 24{\cdot}46$, and $\dot{H} = 7{\cdot}52$ grs.

corrected by A. these two analyses give

	B.	C.	($C_{40} H_{30} O_6$) gives
Carbon	75·59	75·53	75·83
Hydrogen	9·47	9·35	9·28
Oxygen	14·94	15·12	14·89
	100	100	100

It is remarkable, if the experiments above detailed are to be depended upon, that the only difference in constitution between resins B. and C. consists in the presence in the former of thirty-one, and in the latter of only thirty equivalents of hydrogen, while their respective solubilities in alcohol, their fusibilities and general relations to heat are so very dissimilar. We know as yet too little of their several relations to other substances to enable us to speculate on the mode in which the elements are grouped in the two compounds.

Conclusions.—The sandarach of commerce consists of three resins, all of which possess acid properties.

1. $A = C_{40} H_{31} O_5$ a white or yellow powder, sparingly soluble in alcohol, more so in boiling than in cold, melting with difficulty, and decomposing at a temperature little above that at which it melts, existing in sandarach only in a small proportion, and partly separated on treating the natural resin with a large quantity of alcohol, and partly thrown down from this solution of the natural resin by caustic potash along with the insoluble resin C.

2. Resin $B = C_{40} H_{31} O_6$ bright-yellow, brittle, softening at 212° Fahr., having the resinous odour of sandarach, dissolving largely in cold alcohol, constituting at least three-fourths of the natural resin, and remaining in solution when A. and C. are precipitated by caustic potash.

3. Resin $C = C_{40} H_{30} O_6$ a pale-yellow powder, nearly soluble in boiling alcohol, requiring a high temperature to melt it, and at the same time undergoing decomposition. It exists in sandarach in much larger quantity than resin A., and is obtained in a pure state by decomposing with muriatic acid the precipitate thrown down by caustic potash, and boiling first in water and afterwards in repeated portions of common alcohol.

I shall have occasion hereafter to describe several other nearly insoluble resins, differing in composition from those above described, but I may here introduce the remark, that we are already prepared to anticipate unlike *rational* formulæ for the soluble and insoluble resins; since the number of atoms of oxygen contained in our *empirical* formulæ appear to have no relation to these properties of solubility and fusibility. This is strikingly shown in the case of the resins above described, and there are many others in which this fact is equally apparent.

Note.—The disagreement between some of the details of the above examination of the resin of Sandarach, and those of Unverdorben, to whom we are indebted for the only previous methodical examination of a large number of the resins, is to be ascribed chiefly I believe to the mode in which Unverdorben examined the resins. Being unable to have recourse to the ultimate analysis of the substances he obtained, he distinguished them chiefly by their physical characters, which in many cases are very fallacious. It is possible, however, that different specimens of the resin may contain the several resins in different proportions.

VII. *Resin of the Pinus Abies or Spruce Fir, sometimes called Thus, or Common Frankincense.*

This resin is brittle but soft, and easily scratched by the nail; of a pale-yellow colour, and semitransparent. It occurs in rounded masses of various sizes, mixed with chips of wood. When melted in hot water and strained through a cloth, it forms the Burgundy pitch of commerce. In cold alcohol it dissolves readily, leaving a variable proportion of a second resin B. in pure white opaque masses. This second resin is not insoluble in the cold, but only more sparingly soluble than resin A, and readily dissolves in hot alcohol. It should therefore be washed with several affusions of cold alcohol, and dried by strong pressure between folds of bibulous paper. Thus obtained and dried it is in the form of a white farinaceous powder, which requires a temperature of 300° + Fahr. to bring it into complete fusion; and on cooling from fusion it is pale-yellow, transparent, and brittle, differing little in appearance from resin A. after it has been similarly fused.

I. *Resin* A.—The solution of the soluble resin was evaporated, and exposed in a thin film for thirty-six hours to a heat not exceeding 200° Fahr.; it was still soft at that temperature, and on cooling became brittle, but did not contract and crack. In this state,

A. 12·288 grains gave $\ddot{\mathrm{C}} = 33·67$, and $\dot{\mathrm{H}} = 10·35$ grains.

A portion of the resin thus analysed was again heated in a thin film for eighteen hours at a similar temperature. It was still beautifully transparent and pale-yellow, and on cooling contracted and cracked in various directions. In this state,

B. 11·63 grains gave $\ddot{\mathrm{C}} = 31·98$, and $\dot{\mathrm{H}} = 9·731$.

Another portion of the alcoholic solution was evaporated and heated as before for a still longer continuance, and to a temperature a little higher. It began now to assume a reddish tinge on the surface, and, on cooling, the fissures which traversed the yellow mass were of a beautiful red colour. Again analysed,

C. 12·35 grains gave $\ddot{\mathrm{C}} = 34·083$, and $\dot{\mathrm{H}} = 10·238$.

These three analyses are equal respectively to

	A.	B.	C.
Carbon	= 75·76	76·01	76·31
Hydrogen	= 9·35	9·29	9·21
Oxygen	= 14·89	14·70	14·48
	100	100	100

I consider the state of the portion employed for the second analysis B. to represent most nearly the normal condition of the resin, and the result of this analysis agrees almost exactly with the formula $C_{40}\,H_{29}\,O_{6}$. Thus

40 Carbon	= 3057·480	= 76·01
29 Hydrogen	= 364·919	= 9·07
6 Oxygen	= 600·000	= 14·92
	4022·399	100

The results of the whole three analyses are very near these calculated numbers, and yet they illustrate very distinctly what I have frequently had occasion to remark in the study of the resins, that we can rarely depend on the results of any one analysis, however carefully performed. To obtain the resin in a normal state, free on the one hand from adhering water, alcohol, or ether, and on the other from any carbo-hydrogen or other compounds which may be present in it in its natural state, it must be heated for a length of time, and yet neither so long nor to so high a temperature as to induce incipient decomposition. Much attention therefore is required to the process of heating; and when this attention is paid, some change of appearance will always be observed which may be presumed to indicate decomposition; still where doubt remains, or where a formula cannot be obtained, a series of analyses of the resin, in its several stages of heating or drying, can alone determine what numbers are to be considered as expressing most nearly its true elementary constitution.

II. *Resin* B. *of the Pinus Abies.*—I have already stated that cold alcohol leaves undissolved a white opake portion, in greater or less quantity, when the natural resin is digested in this liquid. By repeated solutions in hot alcohol, partial evaporation, and pressure between folds of bibulous paper, this resin may be obtained nearly free from the more soluble resin A. The three portions subjected to analysis were prepared from as many separate quantities of the natural resin, and the constancy of the results may be considered as indicating the degree of confidence to be placed in the formula deduced from them.

A. 12·247 grains heated till it was completely melted and began to assume a reddish hue, gave $\ddot{C} = 34·35$, and $\dot{H} = 10·37$ grains.

B. 11·94 grains heated only till it cohered, and was brittle when cold, gave $\ddot{C} = 33·767$, and $\dot{H} = 10·42$ grains.

C. 8·495 grains after perfect fusion gave $\ddot{C} = 24·02$, and $\dot{H} = 7·18$ grains.

These results are equal to

	A.	B.	C.
Carbon =	77·55	78·20	78·182
Hydrogen =	9·40	9·69	9·390
Oxygen =	13·05	12·11	12·528
	100	100	100

And they indicate the formulæ $C_{40}\,H_{29}\,O_5$, which gives

$$\left.\begin{array}{ll}\text{Carbon} & = 77{\cdot}95\\ \text{Hydrogen} & = 9{\cdot}30\\ \text{Oxygen} & = 12{\cdot}75\end{array}\right\}100.$$

Conclusions.—The resin of the *Pinus Abies*, therefore, consists of two acid resins, represented respectively by

$A = C_{40}\,H_{29}\,O_6$, easily soluble in cold alcohol, fusible about 212° FAHR.

$B = C_{40}\,H_{29}\,O_5$, sparingly soluble in cold alcohol, fusible at 300° FAHR.

The presence of a second and crystallizable resin in this natural product has been already observed by BAUP*; and it is probable that many other of the resins which exude from trees belonging to the pine and its kindred tribes, contain, in like manner, a crystallizable resin possessed of a peculiar elementary constitution. These two classes of resins have hitherto been distinguished only by the general names of *pinic* and *sylvic* acids, though the several members of each class are at least as different in composition as the two resins themselves, which have been regarded as the types of each class.

This observation, suggested by the examination of the natural resin of the *Pinus Abies*, is confirmed by an analysis of HESS†, of a crystalline resin which he obtained as sylvic acid, but which he found to be composed of $C_{40}\,H_{30}\,O_8$, and for which, though he is unacquainted with the natural resin from which it was extracted, he has proposed the name of *Oxysilvic*. This name it is obvious would apply to the crystalline resin of the *Pinus abies*; it is therefore desirable that some other more general nomenclature for this class of bodies should be devised.

The quantity of the crystallizable resin which they contain varies in different portions of the natural product. Such is the case with elemi resin, of which some specimens dissolve completely in cold alcohol; such is the case with colophony; and such also is the case with the resin above analysed. In examining a mass of the natural resin, parts of it will be found to be harder than the rest, whiter, having a lamellar fracture, being tough and yet friable; in these portions the crystallizable constituent exists in greatest quantity. If the action of the air have any influence in changing the chemical constitution of the natural turpentines as they exude from the trees, it is not difficult to understand either how one portion should combine with five and another with six of oxygen, or how the quantity of each of these compounds present in any two masses should be found to be very different.

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* Annales de Chimie et de Physique, tom. xxxi. p. 108. He calls it *abietic acid*, and describes it as an acid resin soluble in 7·5 of common alcohol (88 per cent.) at 57° FAHR., and crystallizing in quadrangular plates.

† POGGENDORF's Annalen, tom. xlvi. p. 326.

The solutions of both the resins of *Pinus abies* in alcohol gave precipitates with an alcoholic solution of acetate of lead, but I have not examined any of their salts.

VIII. *Resin of Olibanum, the Frankincense of the Ancients.*

When the olibanum of the shops, consisting of rounded masses, differing to the unpractised eye only in depth of colour, was introduced into alcohol, some pieces were rendered clear and transparent, while others became almost immediately white and opake, from a white powdery coating left on their surface as the soluble portion is taken up by the fluid. That the mixture of these two varieties had not been made recently, appeared from the manner in which pieces of the different kinds cohered and were occasionally imbedded in each other, as if the contact had taken place while they were comparatively soft.

I. Before I perceived this difference a considerable portion was already dissolved. I therefore filtered the solution, and evaporated a portion of it in a thin film. The two varieties also were separated from each other, and covered each with fresh alcohol.

Of the resin obtained by evaporating the solution of the mixed varieties,

11·236 grains gave $\ddot{C}$ = 30·72, $\dot{H}$ = 10·40, or per cent.,

Carbon = 75·59 = 40 atoms.
Hydrogen = 10·28 = 32·2 atoms.
Oxygen = 14·13 = 5·71 atoms.
100

II. The lumps which had retained their transparency when first digested in cold alcohol, were acted on more slowly by this menstruum than the other variety. They however after a time became covered with an opake white coating, but it was less in quantity than the similar insoluble matter left by the other. The pale-yellow solution was evaporated in a thin film, and kept for sixty hours at a temperature of 200° Fahr. The resin was pale-yellow, brittle, and cracked on cooling, and became soft at 220° Fahr.

A. 13·1 grains gave $\ddot{C}$ = 37·58, and $\dot{H}$ = 12·69.

B. Another portion of the same solution evaporated and heated for eighteen hours, gave a resin of which 10·17 grains gave $\ddot{C}$ = 29·047, and $\dot{H}$ = 9·767.

9·695 grains gave $\ddot{C}$ = 27·73, and $\dot{H}$ = 9·376.

These are equal to

	A. (2.)	B. (3.)	C. (4.)
Carbon =	79·33	78·98	79·09
Hydrogen =	10·76	10·67	10·73
Oxygen =	9·91	10·35	10·18
	100	100	100

These results agree very nearly with the formula $C_{40}\ H_{32}\ O_4$, being that originally adopted by ROSE to represent the constitution of colophony. This formula gives

40 Carbon =	3057·480 =	79·27
32 Hydrogen =	399·347 =	10·36
4 Oxygen =	400·000 =	10·37
	3856·827	100

The most marked disagreement is in the amount of hydrogen which by analysis is about one tenth per cent. greater than it should be, allowing for the usual errors of analysis. The formula $C_{40}\ H_{33}\ O_4$ would give 10·6 per cent. of hydrogen, which is probably too near the quantity found to represent the amount actually present in the resin.

At the same time I would take the present opportunity of repeating*, that in none of the formulæ deduced from the analyses detailed in the present investigation, is the number of equivalents of hydrogen to be considered as *absolutely* determined. Whatever precautions may be taken to obtain any given resin in a *normal* or perfectly pure state, and to avoid the ordinary errors of experiment, the examination of every new member of this interesting family convinces me that though the formula deduced from analysis may express very truly the relations between the number of the equivalents of carbon and oxygen, yet that the hydrogen is doubtful to the extent of one or two equivalents always in excess, and that to this extent, therefore, the formula I have given may be still open to correction.

III.—1. Of that which became soonest opake from containing the largest quantity of gum, two several portions of the solution were evaporated in thin films, and heated for sixteen hours at 250° FAHR.

A. Of the first portion 10·22 grs. gave $\ddot{C}$ = 28·67, and $\dot{H}$ = 9·575 grs.

B. Of the second portion 6·33 grs. gave $\ddot{C}$ = 17·693, and $\dot{H}$ = 5·908 grs.

These are equal to

	A. (5.)	B. (6.)
Carbon =	77·57 =	77·29
Hydrogen =	10·41 =	10·37
Oxygen =	12·02 =	12·34
	100	100

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* Ante, p. 286.

The formula $C_{40}\ H_{32}\ O_5$ gives

Carbon = 77·28
Hydrogen = 10·09
Oxygen = 12·63
100

2. Exhausted with more alcohol, this portion gave a resin of a different constitution, which did not harden so readily on cooling even after long heating, and was obtained brittle only in very thin films.

C. 10·435 grs. gave $\ddot{C}$ = 28·39, and $\dot{H}$ = 9·35.
D. 5·67 grs. gave $\ddot{C}$ = 14·49, and $\dot{H}$ = 4·867 or

	C. (7.)	D. (8.)
Carbon =	75·23 =	74·66
Hydrogen =	9·95 =	10·07
Oxygen =	14·82 =	15·27
	100	100

The formula $C_{40}\ H_{32}\ O_6$ requires

Carbon = 75·36
Hydrogen = 9·84
Oxygen = 14·80
100

These discordant results seemed to imply that this gum resin contains two resins, of which the one is represented by $C_{40}\ H_{32}\ O_4$, and the other by $C_{40}\ H_{32}\ O_6$. This would account for the formula $C_{40}\ H_{32}\ O_5$ given by the above analyses A. and B., on the supposition that the resin employed in these analyses was a mixture of the two, as well as for the occurrence of the resin with four equivalents, *alone*, in some varieties of olibanum.

3. With the view of satisfactorily determining this point, a further quantity of olibanum was immersed in alcohol, and the portions which soonest exhibited a coating of gum were selected. Treated with cold alcohol for several days with occasional agitation, these gave a pale yellow solution which was decanted. The undissolved part was boiled with more alcohol, and a much paler solution obtained. This paler solution evaporated in a thin film, as usual, gave a brittle resin, having the colour and much of the smell of colophony, of which

E. 7·20 grs. gave $\ddot{C}$ = 19·51, and $\dot{H}$ = 6·43 or per cent.

Carbon = 74·93
Hydrogen = 9·92
Oxygen = 15·15
100

which agrees with the results C. and D., and leaves no doubt that a resin with six atoms of oxygen is one of those which exist in olibanum. The first, or yellower solution, gave a softer resin, the analysis of which was lost by a failure in the burning.

4. On examining the olibanum of commerce after the above experience of the presence of different resins, it is easy to distinguish by the eye at least two varieties; one in rounded brittle, brownish-grey, opaque masses of various sizes; the other in long tears as if it had flowed more easily from the tree, and been less rounded by attrition, often softer, less brittle, and less dull in aspect. There occur also other pieces not to be classed with either, the composition of which can be determined only by a separate examination *. The first variety described is that which contains most gum.

A few pieces of this were carefully selected and treated with alcohol for several days in the cold till the resin was entirely exhausted. The *whole* of the solution was evaporated together, and the resin heated at 250° Fahr. for sixteen hours.

8·03 grs. gave $\ddot{C} = 21·87$, and $\dot{H} = 7·215$ or

	(10.)	$C_{40} H_{32} O_6$ gives
Carbon =	75·31	75·36
Hydrogen =	9·98	9·84
Oxygen =	14·71	14·80
	100	100

These selected portions, therefore, consisted wholly of gum in large quantity, and of a resin containing six of oxygen; and as we have already seen that those pieces which include less gum, consist wholly of gum and a resin containing only four of oxygen, and having much of the smell and other properties of colophony, I have thought it unnecessary to search for any other explanation of the discordant results exhibited in analyses 5 and 6, than the supposition that the resin employed in these analyses was a mixture of the two. Had I chosen to omit these two analyses from the present paper, no discordance would have appeared among the results detailed. I think it better, however, that in an investigation like the present, no important steps should be omitted, both because they illustrate the precautions it is necessary to adopt in order to obtain a pure substance, and because others who may repeat my experiments, should they obtain repeated results agreeing so closely, and indicating so distinct a formula as the analyses 5 and 6 (above), might consider themselves justified, without further examination, in pronouncing me to have been in error.

Conclusions.—The gum resin olibanum of commerce consists of a mixture of at least two gum resins, the resinous ingredient in each of which differs from that of the other in composition and properties.

1. The rounded, opaque, dull, hard, and brittle pieces, which speedily become covered

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* These remarks I believe to apply generally to the olibanum as it is met with in this country, the first quantity I examined being bought in Edinburgh six years ago, the second in London within these few months.

with a white crust when immersed in alcohol, contain an acid resin (A.) $= C_{40}\,H_{32}\,O_6$. This variety constitutes the larger portion of the olibanum of commerce, and is the more fragrant when burned. Its composition is determined by the four concordant results (7, 8, 9, 10). It contains a variable quantity of a volatile oil, as is the case with nearly all the resins, by which some specimens even after long heating are prevented from immediately becoming brittle when allowed to cool.

2. The clearer, yellower, less brittle and opaque pieces, generally in long tears (stalactitic ?) as they have flowed from the tree, and which give less gum when treated with alcohol, contain a resin represented by $C_{40}\,H_{32}\,O_4$, and having considerable resemblance in smell and in its other properties to colophony.

Whether these two varieties issue from different parts of the same tree, or at different seasons, or whether the true olibanum is mixed with the produce of another tree, it is not easy to determine. We can scarcely believe that the two varieties issue from the same tree under the same circumstances.

As I have already said, there may be other resins mixed with these in smaller quantity, in the olibanum of commerce, in regard to the composition of which I have made no examination.

General Observations.

We have already seen in the first and second parts of this inquiry, that the resins may differ very widely in the number of equivalents, not only of oxygen, but also of hydrogen, which they contain ; and founding on this difference in respect of the hydrogen, we have seen reason also to divide such as have hitherto been analysed into two groups, represented respectively by the general formulæ

$$C_{40}\,H_{32-x}\,O_y \text{ and}$$
$$C_{40}\,H_{24-x}\,O_y.$$

The resins described in the present paper belong to the first of these groups, and illustrate very clearly the differences which may exist in the number of equivalents of hydrogen among the several members of the same group. A comparative inspection of the formulæ is sufficient to satisfy us that such differences exist in nature.

Resin of *Pinus abies* B $= C_{40}\,H_{29}\,O_5$
Resin of Sandarach A $= C_{40}\,H_{31}\,O_5$

Resin of *Pinus abies* A $= C_{40}\,H_{29}\,O_6$
Resin of Sandarach C $= C_{40}\,H_{30}\,O_6$
Resin of Sandarach B $= C_{40}\,H_{31}\,O_6$
Resin of Olibanum A $= C_{40}\,H_{32}\,O_6$.

At present I do not dwell on these partial generalizations, as the results of other experiments which I shall hereafter have occasion to detail will render necessary a slight modification of one or both of the above general formulæ.

Durham, May, 1839.

XIX. *Researches in Embryology.—Second Series.* *By* Martin Barry, *M.D. F.R.S.E., Fellow of the Royal College of Physicians in Edinburgh. Communicated by* P. M. Roget, *M.D. Sec. R.S.*

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IN a former communication† I described the mammiferous ovum in its several periods of formation. The present paper is intended to trace it through the early stages of development.

What is known, or supposed to be known, of the evolution of the mammiferous ovum in its early stages, mainly rests on observations made, not in Mammals, but in Birds. Direct observations have been so few and so isolated, that between the time when the coitus takes place, and that of the incipient appearance of the vertebræ, there exists a dark period of which very little is really known. This hiatus it is one of the objects of the present paper to assist in filling up.

The ova of the Vertebrata generally were the subjects of investigation in my last memoir, it being then intended to demonstrate the essential identity in form, of certain structures which exist in the four classes of vertebrated animals. The ovum as then considered was in a state of comparative, though not entire, repose. The present series of researches, on the contrary, investigates the ovum while undergoing rapid changes. In order the more completely to watch the progress of these changes, it seemed desirable to confine the attention to a single species. The one I selected was the Rabbit, to which animal all general remarks made in this paper will refer‡.

For the illustration of a subject like the present, which may be compared to a series of metamorphoses, it will be obvious that we ought to be in possession of a *suite* of stages. These I trust have been obtained, though not without difficulties at times in the highest degree discouraging and inseparable from the very nature of the subject,—difficulties felt by De Graaf, Cruikshank, Haighton, Prevost and Dumas, Baer, and others. But there existed other difficulties, the best evidence of which is the result, that I am compelled, however reluctantly, to call in question some views which were considered to be settled; and these embracing points of cardinal importance. For example, according to my observations the embryo does not arise in the substance of a membrane.

The discrepancies to be found in the accounts of authors on the ovum of Mammalia, and the confused nomenclature of its membranes, appear referable to the absence of early observations in continuous succession. Observers have not directed their attention to the ovum *post coitum* before it leaves the ovary; and they cannot be said to have thoroughly examined it in the Fallopian tube. Now so rapid are the changes which the ovum undergoes at the period in question, that had I not very often seen it in both of those localities, it would have been impossible for me at least to understand it in the uterus. And in proof that others failed to do so, it may be stated that ova of the Dog measuring half a line, have been described by eminent observers as consisting of a single membrane; whereas in the Rabbit, according to my observations, ova of still minuter size may consist of four membranes in addition to the embryo, (one of these membranes being composed of two laminæ); to which it may be added, that I find one membrane to disappear by liquefaction within the ovum while the latter is in the Fallopian tube.

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† Researches in Embryology, First Series. Philosophical Transactions, 1838, Part II. p. 301.

‡ Among the figures will be found several from the Tiger, for an ovary of which animal I am indebted to the kindness of Professor Owen.

From what has been stated, it will be obvious that in undertaking researches in so difficult a subject as that of embryology, the mind should not be pre-occupied with any theory. I certainly had none to establish. If I was at all prepossessed, it was in favour of existing doctrines; and it has been in spite of such prepossession that fact after fact has gradually but irresistibly compelled me to form other views. In the following pages there will be found no speculations, as I have confined myself to a simple delineation of appearances, and to the most obvious conclusions arising from them. If in any points I have been mistaken, that indulgence will, I trust, be accorded which may be fairly claimed in exploring one of the most hidden regions of physiology. That in the main facts I have not erred seems probable, from the repeated opportunities afforded for their confirmation. The number of Rabbits examined considerably exceeds a hundred, and I have recorded in my notes the particular results regarding eighty-nine. Besides ova that were still present in the ovary and apparently destined to escape, ninety-three have been found in the Fallopian tube, and two hundred and thirty-six in the uterus. I have kept a separate record of the measurements and other particulars of most of these. Tables will be given (par. 319.), showing the diameter, general condition, and locality, of two hundred and fifty-six of the minuter of these ova, very few of which exceeded in diameter half a Paris line, and by far the greater number were considerably below this size, some indeed not exceeding $\frac{1}{14}$th and even $\frac{1}{15}$th of a Paris line. Of these ova I have preserved sixty-six. The foregoing will serve to show that my conclusions have not been drawn from solitary facts or isolated observations†.

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† The following testimony to the importance of the history of development, is from the pen of an eminent observer, (E. Huschke, in Meckel's Archiv, vol. vi. p. 1.) "Systematic physiology rests especially on it, and can never rapidly advance unless it becomes more perfect, for this it is which gives to the philosopher the material wherewith to rear a solid fabric of organic life. Hence in anatomy and physiology, our endeavours ought more than is now the case to have reference to it; in other words, we should constantly examine not only every organ, but every material, and also every action, with the inquiry, *how did they originate?*"

(The measurements throughout this paper, as in the former one, are stated in fractions of a Paris line, and thus expressed (‴). As a simple mode of reducing this fraction into (what is very nearly) the equivalent fraction of an English inch, I recommend multiplying the denominator of the former by 11½. See the "Table of Measurements" (par. 320.). The actual sizes of some of the ova are represented at the foot of Plates VI. and VII.

The Mature and the Immature Ovum.

120. The difference perceptible between ova in these two states appears to me to consist in the condition of the yelk. In the immature ovum the yelk contains separate oil-like[†] globules, diffused in a fluid (Plate V. fig. 85. *d*); while in the ovum that is mature, the yelk presents a peripheral stratum, sometimes appearing granulous, and at others seeming to consist of vesicles pressed together into a polyhedral form, its centre being in the state of fluid.

121. The globules of the yelk just mentioned as oil-like in their appearance in the immature ovum (fig. 85. *d*), are in reality vesicles. In fig. 87. are some of those vesicles from an immature ovum of the Tiger, in which animal I found them exceedingly distinct. But besides being true vesicles, those globules contain objects which themselves are also vesicles; the latter in some instances presenting opacities in their interior, and they are often observed to be pressed into an irregular shape[‡].

122. The globules contained in the vesicles ("cells") represented by Dr. SCHWANN[§] from the yelk-cavity of a Hen's egg do not exhibit the appearance of vesicles so decidedly as the above. In the Cat, however, I have found the vesicles of the yelk to contain a globule very much resembling that just referred to as figured by SCHWANN, yet the outer vesicles had the same high refracting power as those from the Tiger (Pl. V. fig. 87.), and in this respect, therefore, differed from those in the figure by SCHWANN. Is the difference referable to a difference in age[‖]?

Effects produced on the Ovum in the Ovary by Maceration.

123. On a former occasion (*l. c.* par. 50.) I referred to the effects of maceration, as increasing the probability that a proper membrane of the yelk existed generally in the class Mammalia; for it was shown that although the thick transparent membrane of the ovum or "zona pellucida" (*f*) by imbibition had become distended, the yelk-

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[†] The term "*oil-like*" is used, as in my former memoir, to describe simply the appearance and not the nature of the globules to which it is applied.

[‡] It is an interesting fact, as several figures in my "First Series" (*l. c.* Plate V. figs. 14, 15, 16,*) serve to show, that the yelk-globules (vesicles) collect, and perhaps originate, around *the germinal vesicle.*

[§] Mikroskopische Untersuchungen über die Uebereinstimmungen in der Struktur und dem Wachsthum der Thiere und Pflanzen. Tab. II. fig. 2. Berlin, 1838–9.

[‖] Perhaps, however, it is the globules of the "*discus vitellinus*" rather than those of the yelk-cavity in the egg of the Bird, with which the globules of the substance usually called the yelk in the mammiferous ovarian ovum are to be compared (par. 174. first Note. par. 318.).

ball still retained its size and form. In the present paper, Plate V. fig. 88. affords a proof of this in an ovum of the Tiger. Here, however, in addition to the yelk continuing spherical, its proper membrane (*e*) was very distinctly visible.

124. The most conspicuous change produced by maceration is distention of the membrane *f*, but I have observed the germinal vesicle also before disappearing to become elliptical. See the figure just referred to and also fig. 89., which exhibits the germinal vesicle (*c*) of the same ovum on a larger scale. From the germinal spot (*b*) in this instance there were seen to have arisen three membranes or vesicles, its central part consisting of a fourth, which appeared to contain a pellucid fluid. Between these membranes or vesicles of the former spot, were granules and a fluid (par. 298.) the granules being situated for the most part near the centre.

The Rut.

125. It is known that at this period there occurs increased vascularity of the parts in general, and of certain Graafian vesicles in particular. I have found that the number of Graafian vesicles appearing to become prepared, by enlargement and vascularity, for discharging their ova, exceeds the number of those that actually discharge them (par. 126.). The fluid of the Graafian vesicles in general which are situated at the surface of the ovary, is more viscid now than at other seasons. The degree of this viscidity, however, as well as the condition of the yelk, seems to depend on the degree of advancement of the period in question. In ova taken from enlarged and vascular Graafian vesicles, the germinal vesicle is at the periphery, and the yelk mature (par. 120.). I have also in some instances observed the peripheral stratum of the yelk to have become more finely granulous than in usual states of the mature ovum *ante coitum*. The changes in the tunica granulosa and retinacula, incipient perhaps at this period, will be more particularly mentioned among those referable to the coitus.

The Ovum in the Ovary post coitum†.—*First and Second Stages of Development.*

126. I have found it extremely difficult to distinguish with precision between the changes referable to the rut, and the further changes resulting from the coitus. For reasons given in the introduction to this memoir, it did not seem sufficient to see the state of ova first in the Fallopian tube. But then it was equally unsatisfactory to examine ova still in the ovary; for at first I was not in possession of any means of distinguishing those that were really destined to be discharged, as in addition to the Graafian vesicles, from which ova would have been expelled, I have (as already stated)

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† As researches of this kind would be impossible, or the results of no value, without the means of determining precisely the period between the coitus and the death of the animal, I shall be excused if I mention for the professional reader, 1. That persons were employed who might be relied upon, and were fully competent to judge of the condition of the female rabbit in reference to the rut: 2. That the coitus was in every instance *seen*, and the time as well as the degree of readiness for the reception of the male particularly noticed and recorded: and 3. That immediately after the coitus the female Rabbit was removed and kept separate from the male.

generally found several others that had become enlarged and vascular (par. 125.). It therefore was an object of great interest to meet with ova that were on the point of entering the tube.

127. The periods of 4, 6, 8, and $8\frac{1}{2}$ hours *post coitum*, were found too short, the ova being still within the ovary, and apparently not very near the time of their expulsion. A rabbit was examined at eleven hours, when the ova were found to have made their exit from that organ; and one at ten hours, with the same result. Another was killed at nine hours; the ova were still within the ovary, and their Graafian vesicles presented no decided indication of an approaching rupture. I tried $9\frac{1}{4}$ hours, when the ova were again found within the ovary; while in another instance at nine hours they had escaped. This was discouraging, but it seemed worth while to persevere. At length, after nearly a score of rabbits had been devoted to anatomical inspection, for the single object of determining the condition in which the ovum leaves the ovary, the parts were found in a state precisely what I had so much desired to meet with; one of these animals at ten hours yielding me two ova that had left the ovary and advanced an inch into the tube, and two others that were still in the ovary, but beyond all doubt on the point of following them.

128. According to my observations, then, ova of the Rabbit destined to be developed are frequently discharged from the ovary in the course of nine or ten hours *post coitum*. Burdach† refers to only one authority on this subject, stating that Cruikshank saw the Graafian vesicles of the Rabbit to burst in the short period of two hours. From Cruikshank's paper‡, however, it appears that this was observed in a single instance only. Kuhlemann§ found Graafian vesicles of the Sheep to have burst at the end of the first day; and Hausmann‖ observed that in the Hog they had nearly all burst in seventeen hours. The conclusions of Prevost and Dumas¶ as to the time of bursting of the Graafian vesicles in Dogs, appear to rest chiefly on the period when those observers found ova in the uterus, from which nothing can be determined as to when they left the ovary. I have recorded in my notes several instances in which, after a much longer period than nine or ten hours, the ova (in the Rabbit) had not escaped. But in those instances there was no proof that any ova were destined to escape,—in other words, that the coitus had been productive. Still, however, the condition of the animal, and more particularly the degree of its advancement in the rut, probably occasion considerable differences in this respect. Yet I am satisfied that ova very frequently leave the ovary in nine or ten hours; to have determined which, it will be hereafter seen (par. 166.) is a point of some importance.

129. Several observers appear to have supposed the ova to be still present in the ovary after their escape. Cruikshank††, for instance, mentions a little body which he found on the top of the *corpus luteum* three days *post coitum*. That little body he

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† Die Physiologie als Erfahrungs Wissenschaft, vol. ii. p. 12.

‡ Philosophical Transactions, 1797.

§ Burdach, *l. c.* vol. ii. p. 12.

‖ Ibid. vol. i. p. 555.

¶ Froriep's Notizen, No. 189, p. 198.

†† *L. c.* p. 206.

supposed to be the ovum. What it really was I shall endeavour hereafter to render probable (par. 155.); but it is very unlikely to have been an ovum.

130. It appears from the observations just made known (par. 127.), that in the Rabbit all the ova of one impregnation are discharged at about the same time. This is opposed to the opinion of PREVOST and DUMAS[†], that in the Rabbit at least two days elapse before all the ova destined to be developed have left the ovary. That I am correct in this particular, is rendered still more probable by the fact, that in the Fallopian tube, and in the beginning of the uterus, minute ova are generally (though not in every instance) met with lying very near together[‡]. Having found more than three hundred ova in the uterus and tube, I may perhaps venture to speak with some confidence as to their particular localities, which it has uniformly been my practice to record. A general idea of the situation in which many of the minuter ova have been observed, may be obtained from the Tables (par. 319.).

131. If my endeavours to obtain ova when just on the point of entering the tube were for a long while fruitless, unexpected facts were noticed, amply repaying all the labour. It was in the course of those wearisome endeavours, that the germinal vesicle first presented itself in situations which made me doubt its disappearing at the period hitherto supposed. I saw it in mature ova half-way between the surface and the centre of the yelk,—a locality which was remarkable from the fact, that *ante coitum*, and even while the ovum is yet immature, the germinal vesicle is seen to have passed to the surface of the yelk. Now such a situation of the germinal vesicle would probably have escaped my attention, if the observation had not been many times repeated. Yet on a knowledge of the fact that the germinal vesicle returns to the centre of the yelk, depends the possibility of fully understanding the ovum in some of its future phases.

132. I shall now describe the changes apparently resulting from sexual connexion, observed in the ovum of the Rabbit while still within the ovary. Some minute details will be unavoidable, but so far as is consistent with perspicuity I shall endeavour to be brief.

133. After some hours the germinal vesicle is found to have left its situation at the surface of the yelk and to be *returning to its centre*, from whence it came. About the same time I have observed to have arisen from the surface of the germinal spot a membrane—that is, a vesicle—which speedily enlarged so as to apply itself to the inner surface of the germinal vesicle. The vesicle thus proceeding from the spot, of course imbibed the fluid of the germinal vesicle, which in its new situation appeared finely granulous, and yellowish-brown in colour. The germinal vesicle having now two membranes, was less transparent, and less easily ruptured than before; and I have

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[†] *L. c.* No. 189, p. 199.

[‡] It is an interesting fact, that if a larger number of ova than usual escape from one ovary, a proportionally smaller number is discharged from the other.

observed what appeared to be the further effect of this additional membrane, in the vesicle remaining visible, casting a shadow, and even not at all collapsing after being ruptured (Plate V. fig. 92.).

134. The germinal spot, previously on the internal surface of the germinal vesicle, is soon observed to occupy its centre; presenting thus the same change in relation to the germinal vesicle, as the latter undergoes in reference to the yelk-ball. The spot becomes very much enlarged, and in its centre there is now a pellucid point.

135. On some occasions, when the germinal vesicle was observed to be returning from the surface to the centre of the yelk, the yelk viewed with reflected light exhibited the appearance of masses, between which there was a pellucid fluid. At other times I have seen with great distinctness a cavity in the centre of the yelk. Plate V. fig. 93. shows the germinal vesicle (*c*) just entering such a cavity. This figure represents a stratum, apparently of vesicles, forming the periphery of the yelk. Subsequently this stratum has disappeared (the proper membrane of the yelk having in the mean time thickened), and the yelk has become fluid at its surface; while its central portion, in which the germinal vesicle now lies, has become obscure. In a stage still later more of the yelk has liquefied, and in the liquid are granules, the central part of the yelk having now an ellipsoidal form (Plate V. figs. 96. and 97.).

136. The proper membrane of the yelk was shown in my "First Series" (*l. c.* Plate VIII. fig. 70. *e.*) to be present at an early period in the existence of the ovum, but stated to be from its tenuity in Mammalia generally invisible. At the period at present under consideration it suddenly thickens, highly refracting light (Plate V. figs. 94. 96. 97. *e.*), and is often reddish-brown in colour. This thickening of the proper membrane of the yelk is not among the earliest of the changes resulting from sexual connexion, but follows those above described, and for the most part takes place immediately before the discharge of the ovum from the ovary. The stratum of vesicles referred to in the preceding paragraph (fig. 93.) has now disappeared.

137. The thick transparent membrane of the ovum, or "zona pellucida" (*f*), begins to imbibe fluid and distend, so that a minute space, filled with fluid, is visible between it and the yelk-ball (Plate V. figs. 96. and 97. *e.* and *f.*). This change follows the incipient thickening of the proper membrane of the yelk (*e*), and in some instances is not appreciable until after the ovum has made its exit from the ovary.

138. The tunica granulosa (Plate V. figs. 93. 96. 97. g^1.) was shown on a former occasion (*l. c.* par. 64–71. 88.) to be at first a spherical and subsequently a flattened accumulation, on the ovum, of the peculiar granules, or rather vesicles of the ovisac. At the period in question its vesicles hang less tenaciously together, and frequently appear to be passing into a fluid state. The tail-like appendages, as I have called them, of this tunic are now very distinctly seen to be continued into the four persistent retinacula, evidently contributing themselves to perform the same offices (par. 149—151.) as the latter (fig. 96. g^1.).

139. The retinacula (Plate V. figs. 93. 96. g^2.) (described in my "First Series"), at the period now before us, become enlarged, for which there seems to be a provision

ante coitum in the wrinkled state (as formerly mentioned, *l. c.* par. 86. *Note.*) of their investing membrane. In some instances I have observed a number of minute dark globules to be mixed with the vesicles which form these structures. Sometimes, as in fig. 96. g^2., the retinacula, beginning to liquefy, resemble a collection of pellucid drops of fluid.

140. Most of the changes now mentioned I have found to take place before the ovum leaves the ovary. Yet certain of the later ones in some instances are not observable until the ovum has passed into the Fallopian tube. From my observations, therefore, it appears that there is no condition of the ovum uniform in all respects, which can be pointed out as that in which it is expelled; though the same observations lead me to conclude that Plate V. figs. 96. and 97. present a state which is frequent when the discharge takes place, viz. the germinal spot (*b*) has a central pellucid point; it is situated in the centre of the germinal vesicle (*c*); the latter has a dark contour, and perhaps a double membrane; around it is an ellipsoidal mass; the yelk (*d*) is a fluid containing granules; the proper membrane of the yelk (*e*) has thickened, highly refracts light, and is often reddish-brown in colour; there is a minute space, filled with a transparent and colourless fluid, between this membrane and the membrane *f*; and finally the tunica granulosa (g^1) and retinacula (g^2) present the appearance of incipient liquefaction.

141. A synopsis in my former memoir exhibited seven successive stages in the *formation* of the ovum (par. 292. *Note.*). In the present paper also, it will be convenient to adopt the same plan of considering the ovum in successive stages. These, however, occurring *post coitum*, will be stages of *development*. I propose to consider as the first stage, that represented in Plate V. fig. 93; and as the second stage, the condition which is frequent when the ovum leaves the ovary (figs. 96. and 97.).

142. To obtain these facts, and have it in my power to state them with any degree of confidence, and in the order of their occurrence, has been an undertaking of no common difficulty; and there did not exist, so far as I could discover, any recorded observations belonging to this period that might be taken as a guide.

Locality in which the Ovum is fecundated.

143. As to the particular locality in which the ovum becomes susceptible of development, physiologists are not agreed. Some maintain that it acquires this susceptibility before it leaves the ovary; others that the change is not effected until after its expulsion from that organ.

144. It is not my purpose to discuss the question whether contact of the seminal fluid with the ovum is or is not essential to impregnation. Yet perhaps it may be proper for a moment to refer to the possibility of that contact while the ovum is still within the ovary, this having been denied.

145. **Prevost** and **Dumas**[†] maintain that the Spermatozoa do not penetrate so far as to the ovary, and conclude that in all Mammals impregnation takes place in the horns of the uterus. I do not doubt that the observations of **Prevost** and **Dumas** were accurate, for in seventeen out of nineteen instances in the Rabbit, though the parts were generally examined while still warm, I was unable to discover Spermatozoa in the fluid collected from the surface of the ovary. In the other two instances, however, Spermatozoa, or at least animalcules exactly like those I had been accustomed to meet with in the uterus and vagina, were really found on the ovary. I should rather say, that on one of those occasions Spermatozoa were seen, while on the other it was a single Spermatozoon that was observed. Some of the former were alive and active, though not in *loco*motion; others were dead. In that case, twenty-four hours *post coitum*, there was neither enlargement of the Graafian vesicles nor a high degree of vascularity in any of the parts. In the other instance the single Spermatozoon found was dead, and the ova had escaped. Now whether the Spermatozoa are essential to the impregnating power of the seminal fluid, I do not think it needful to inquire. The fact that in the course of these researches they have been met with on the ovary, demonstrates that the seminal fluid sometimes penetrates as far as to the surface of that organ. Whether it penetrates into its interior I am unable to determine; but certainly the changes above described, as taking place *post coitum*, in the condition of the ovum while still within the ovary, are too remarkable not to favour the supposition that it does[‡].

146. The changes now more especially referred to will presently be seen. The germinal vesicle *ante coitum*, after the formation of the yelk has begun, is situated in the centre of the latter. From that locality it passes to the surface of the yelk, the germinal spot being situated on the internal surface of the germinal vesicle. The ovum (as shown on a former occasion, *l. c.* par. 85.) is conveyed from the centre to the surface of the Graafian vesicle, and indeed to that part of the surface which is situated nearest to the exterior of the ovary[§], being determinately held by the retinacula in this situation[‖]. The proper membrane of the yelk is hitherto extremely thin. Such is the condition of the mature ovum *ante coitum*; that is to say, its essential parts lie as *near* as possible to the surface of the ovum. *Post coitum*, before the discharge of the

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[†] *L. c.*, No. 189, p. 199.

[‡] Since the above memoir was presented to the Royal Society I have learnt that Professor **Bischoff**, of Heidelberg, had previously found Spermatozoa on the ovary of another Mammal, the Dog (par. 278.).

[§] In repeated instances also I have found the germinal vesicle at that part of the surface of the yelk which was situated nearest to the periphery of the Graafian vesicle, and therefore as near as possible to the surface of the ovary. This accords with the observations of R. **Wagner**, that in *Dytiscus marginalis*, and in some other insects, the germinal vesicle always appears on that side of the ovarian tubes which is directed towards the cavity of the abdomen. Thus in a bunch of ovarian tubes the vesicle is never situated at that part where the tubes lie one upon another. (Beiträge zur Geschichte der Zeugung und Entwickelung, p. 46.)

[‖] R. **Wagner** suggested that the "disc" (my tunica granulosa and retinacula) might serve to hold the ovum at the surface of the Graafian vesicle, and thus promote impregnation. (Beiträge, &c., p. 39.)

ovum from the ovary, the germinal spot passes to the centre of the germinal vesicle, and the germinal vesicle returns to the centre of the yelk. The proper membrane of the yelk suddenly becomes thickened.

147. Such alterations suggest the probability of some sudden and important change having been effected in the condition of the ovum; and moreover, that which is allowed to be its most essential part, previously as *near* as possible to the surface of the ovum, is now withdrawn as *far* as possible from that surface, by being once more removed to its centre. Nor is it to be forgotten that the proper membrane of the yelk, previously extremely *thin*, has suddenly thickened. These changes, which I have no doubt will be confirmed by future observers, render it highly probable that the ovum has undergone fecundation. The nature of the changes is such as to favour the supposition that they are produced by contact of the seminal fluid with the ovum; and we have seen them to take place within the ovary. I therefore suppose the ovary to be the usual locality in which the ovum is fecundated. Still, however, as we have reason to believe that the ovary, in some animals at least, discharges ova which are not fecundated[†], this change may perhaps in some instances take place in the oviduct.

Discharge of the Ovum from the Ovary.

148. This appears to be effected in part at least, as supposed by VALENTIN, through the operation of a *vis a tergo*, the latter being produced by the exuberant growth of a reddish fleshy mass, which acts through the medium of the fluid of the Graafian vesicle. The particular structure originating that fleshy mass, I shall have occasion to refer to in connexion with the *corpus luteum* (par. 156.).

149. When describing in my "First Series" (*l. c.* par. 80–91.) the offices of the retinacula (Plate V. figs. 85, 86, 93, 96. g^2.), I stated that they appeared first to support the ovum in the centre of the Graafian vesicle, next to convey it to the periphery of that vesicle, and subsequently to retain it in the latter situation,—probably contributing also to attenuate the parietes of the vesicle at a certain part, so as to promote the expulsion of the ovum from the ovary. It remains to notice some other offices apparently performed by these structures.

150. It is the central portion of the retinacula, and not the minute ovum, that presents a surface for the operation of a *vis a tergo*. The retinacula therefore escape with the ovum (Plate V. fig. 96. g^2.); and by their long bands, and the connexion of those bands with the membrana granulosa, render the escape of that important body gradual. They also seem to afford a considerable surface for the operation of

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[†] HAIGHTON showed that corpora lutea formed in both ovaries, although the access of the seminal fluid on one side had been made nearly impossible by obliteration of the tube *ante coitum*. Fœtuses, however, were present only on the unmutilated side. Philosophical Transactions, 1797. Dr. BLUNDELL went farther, obliterating the upper part of the *vagina, ante coitum*, and found that the coitus produced corpora lutea but no fœtuses. But we know that even sexual connexion is not necessary for the production of corpora lutea. See some excellent remarks on this subject by Dr. ALLEN THOMSON, article "Generation," in Dr. TODD's Cyclopædia of Anatomy and Physiology, pp. 465, 466.

those means by which the minute ovum is made to enter the Fallopian tube. And finally, enlarged and in a half fluid state, they appear to be the bearers from the ovary of a substance for the immediate imbibition of the ovum, and probably enter into the formation of the chorion.

151. The tunica granulosa (Plate V. fig. 96. g^1.) appears to assist in all of these offices, and especially in the two last named[†].

The Corpus luteum.

152. When the discharge of the ovum from the ovary is very near, that portion of the Graafian vesicle directed outwards is seen to have been removed, so that little or nothing remains to obstruct the passage of the ovum besides the peritoneum. The peritoneum therefore appears to me to be the part that gives way last.

153. If a Graafian vesicle about to discharge its ovum be carefully dissected out of the ovarium, and so placed that the compressor may act upon it laterally, an appearance is obtained which I have represented in Plate V. fig. 95. This on a larger scale, and after the object has been ruptured by compression, is exhibited in fig. 96. Here *h* is the vesicle, which in my former memoir (*l. c.* par. 1–5. 25.) I described as the true and originally independent *ovisac*; *i* is the covering gradually acquired by the ovisac; the union of the two—according to my observations—forming the so-called Graafian vesicle. At the period now under consideration the covering (*i*) has become a thick and highly vascular mass; and with this change in its covering the ovisac itself has lost considerably in the size to which it had been distended.

154. A few hours after the ovum has been discharged, if lateral pressure be applied, there escapes from the thick and vascular mass (*i*) a minute translucent body (Plate V. fig. 98.), perfectly spherical in form, and having a diameter of less than half a line. This, placed under the microscope, is found to be the *ovisac*, thus easily removed from its covering. In the substance of the latter it will be remembered lie the vessels. The ovisac itself presents no trace of any, though in some instances its substance has seemed to be pervaded by pellucid points. At a certain part of it (see the figure) is the orifice by which the ovum was expelled, its margin bloody. I have found that orifice in several instances to be elliptical, and to measure from $\frac{1}{4}'''$ to $\frac{1}{5}'''$ in length.

155. Several days after the ovum has escaped, there is protruded from the centre of what was formerly the Graafian vesicle, a mammillary process, noticed by several observers, very accurately figured by **De Graaf**, apparently mistaken by **Cruikshank**[‡] for the ovum (par. 129.), and not inappropriately compared to a sort of hernia by **Coste**. The primitive ovisac is at this later period no longer met with in the ovary[§],

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[†] Von Baer mentions that in the Dog the "disc" passes with the ovum into the Fallopian tube. (Lettre sur la Formation de l'Œuf dans l'Espèce humaine et dans les Mammifères, Commentaire, p. 40.)

[‡] *L. c.*, p. 206, third day after the coitus, Experiment xviii. "The pouting part I believe is the ovum, and stands upon the top of corpus luteum. It is very vascular, particularly at its basis." See also Experiment xxiv.

[§] Whether in the interim the ovisac has been absorbed *in situ* or first expelled, I do not know. In the Hog I have found what seemed the remains of ovisacs in the infundibulum.

for the mammillary process appears to consist solely of an inverted portion of the vascular and spongy substance which previously constituted the covering of the ovisac.

156. The obvious conclusion from these observations is, that *the covering of the ovisac* (Plate V. figs. 95 and 96. *i.*) *becomes the corpus luteum.*

157. In making this assertion I am so unfortunate as to find myself again expressing an opinion at variance with that of Baer. I have no doubt that the *corpus luteum* forms in the structure where he exhibits it†; but that structure, I must respectfully maintain, is not, as he calls it, the inner membrane of the Graafian vesicle; for that inner membrane is constituted by the ovisac, which disappears. Dr. Pockels‡ has figured three membranes as entering into the formation of an advanced Graafian vesicle, besides the membrana granulosa; and it appears to me that what he has termed the "Nucleus," "Ovum Graafianum," and "Folliculus," is my *ovisac*,—a structure which it will be remembered was in my former paper followed upwards from the minuteness, in some instances, of $\frac{1}{100}$dth of a line. And it farther appears to me to be this same vesicle (the ovisac) that Dr. Pockels refers to in the Sheep and Goat, as remaining in the incipient *corpus luteum* eight days and more after the expulsion of the ovum from the ovary§. Valentin‖ has very accurately shown the appearance of the *substance* of the *corpus luteum* on a small scale.

Disappearance of Ova post coitum.

158. During the rut, as already mentioned, several Graafian vesicles seem to be prepared by enlargement and vascularity for discharging their contents, besides those from which ova are actually expelled; so that for some hours even *post coitum* it is not easy to distinguish the latter from the former. After the ova have been discharged, therefore, the ovary often presents several Graafian vesicles, which are enlarged and highly vascular. These, as well indeed as many of the minuter ones, seem to be absorbed; and during several stages of that process appearances occur which it may be worth while to mention, as from their resemblance to some of the changes produced by impregnation, they are calculated to mislead.

159. The yelk liquefies. This change is first seen *around the germinal vesicle* (Plate V. figs. 99 and 100.), in which situation also it will be remembered the yelk-globules (vesicles) present their first appearance (par. 121. *Note.*). The germinal

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† Lettre, &c., fig. xiv.

‡ Müller's Archiv, 1836, Heft II. Tab. VI.

§ *L. c.*, p. 203. I am inclined to think that the membrane which, in some instances, is found lining the cavity (when a cavity exists) in the corpus luteum, is no other than the originally independent vesicle called by me the "ovisac." See Figures of the corpus luteum in the human female by Dr. Montgomery (Exposition of the Signs and Symptoms of Pregnancy, &c., 1837.), who has very justly stated that it is not the inner membrane of the Graafian vesicle that becomes the corpus luteum.

‖ Dissertation by Bernhardt, "Symbolæ ad Ovi Mammalium Historiam ante Prægnationem." Vratislaviæ, 1834, fig. xxx.

vesicle collapses, generally becomes elliptical, and more or less thickened (fig. 101. *c.*). The germinal spot appears breaking up, and in its centre is sometimes seen a dark point (fig. 101. *b.*). The "zona pellucida" becomes elliptical, thin, and very much distended (figs. 100, 101. *f.*). The tunica granulosa and retinacula liquefy, leaving the ovum uncovered, or with a few dark globules on its surface (figs. 99 and 100.).

160. Such are the changes observed when absorption is commencing *post coitum*. It will be seen that in some respects they resemble the earliest effects of impregnation, but differ from them in the following; viz. the germinal vesicle does not return to the centre of the yelk; the proper membrane of the yelk does not thicken, and is not even visible; and when the "zona pellucida" is distended, the imbibed fluid mixes with the yelk. The above changes also differ from those described in my "First Series" (*l. c.* par. 60. and Plate VIII. fig. 67.) as accompanying the absorption of ova *ante coitum*, in the condition of the yelk. The yelk was then described as nearly black, from myriads of minute granules and oil-like globules; while in ova absorbed *post coitum* the yelk seems to pass immediately into the state of a colourless and pellucid fluid, whether those ova are mature or immature; for I have met with some ova undergoing absorption at this period, which were exceedingly minute.

Graafian Vesicles containing Blood in their Interior.

161. After the impregnated ova have been discharged from the ovary, some of the larger Graafian vesicles, remaining unbroken, are frequently found to contain a considerable quantity of dark blood, which gives them the appearance of blackish spots. Such spots have been noticed by several authors, who supposed them to indicate the Graafian vesicles from which ova were destined to be expelled. It is not unusual, however, to find Graafian vesicles thus filled with blood in cases where the escaped ova, in number, size, and local situation in the uterus, forbid the supposition that more would have been discharged from the ovary. Of such instances I have recorded many in my notes. For example, in a Rabbit, 108½ hours *post coitum* (Table, par. 313.), ten ova were found distributed throughout the two uteri, having a diameter of $\frac{1}{4}'''$ to $\frac{1}{2}'''$, in the nineteenth and other stages. That a discharge of more ova had been destined, is not at all probable; yet in that instance,—besides the incipient corpora lutea, corresponding in number to the discharged ova,—each ovary presented several large and unbroken Graafian vesicles filled with blood (par. 125. 126.).

Ovisacs found in the Infundibulum.

162. On one occasion, with a high degree of vascularity in all the parts, I found in an ovary of the Hog three ruptured Graafian vesicles, with four apparently on the point of bursting†. Bloody strings of a fleshy substance were hanging at the orifices of two out of the three ruptured Graafian vesicles; and in the infundibulum of this side there were several of the same kind of bloody masses, of a string-like form, sug-

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† None of them were distended beyond a moderate size, and they seemed to be in a healthy state.

gesting the idea of their having been rolled†. In the other ovary two Graafian vesicles were ruptured, having strings of the same kind of bloody substance pendent at their orifices. In the infundibulum of this side were portions of blood-vessels.

163. Some of the string-like masses found in the infundibulum, as well as those pendent at the orifices of the ruptured Graafian vesicles, on being examined with the microscope, presented the following parts, viz.

1. A multitude of elliptic vesicles, varying in size from about $\frac{1}{70}'''$ and less to $\frac{1}{4}'''$, of a greyish colour, and more translucent than the mass in which they lay imbedded.

2. A fleshy mass saturated with blood, in which these vesicles were found, together with portions of large and empty vessels.

3. Shreds of ovisacs, not presenting the same vascular appearance.

164. One of the vesicles just mentioned is represented in Plate V. fig. 102. It was obviously an ovisac in the course of being absorbed. At *g* are its peculiar granules, or rather vesicles, in an altered state. In the interior of each of them is another vesicle, containing a colourless and brightly pellucid fluid, and surrounded by granules. This inner vesicle is the former nucleus in an altered state (par. 297.). At *f* is the membrane, which unaltered is highly transparent, and very thick. It has become distended, wrinkled, and very thin. The yelk (*d*) seems to have passed into a fluid state; *c.* is the germinal vesicle, thickened, and probably double; and *b.* the germinal spot, having a pellucid centre. Here incipient absorption is seen to have produced the same effect upon several parts as impregnation and maceration.

165. The presence of such objects in the infundibulum appears to be not unfrequent in the Hog. I have observed them also in the Cat. To explain the occurrence of ovisacs in the infundibulum, I suppose the rupture of a large Graafian vesicle sometimes to involve the discharge of many minute ovisacs, which escape from the ovary in consequence, and are probably absorbed. I now return to the ovum of the Rabbit.

The Ovum after it has left the Ovary.

166. The diameter of the Rabbit's ovum, when it leaves the ovary, does not, according to my observations, generally exceed $\frac{1}{15}$ of a Paris line, and in some instances it is still smaller. This extreme minuteness renders its discovery very difficult. It is, therefore, important to determine the time when the expulsion usually takes place (par. 128.), for we thus obtain some notion of the distance in the Fallopian tube to which the ova have advanced. And though in different individuals this distance in a given time may not be constantly the same, still even a general idea of it is of no small advantage. We thus diminish that extent of surface, to examine which, in quest of an object so minute, long appeared to me an almost hopeless undertaking. I trust that future observers, through the Tables of observations to be subsequently

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† In connexion with the *rolled* appearance of these masses, I would refer to the muscular state at certain periods of the middle coat of the infundibulum.

given (par. 319.), will be spared in some degree the fruitless labour which, in the absence of such information, it was my misfortune to bestow. An experienced eye may also infer from the condition of the discharged Graafian vesicles or incipient corpora lutea, whereabouts the ova lie. I have already stated that all the ova are discharged from the ovary at about the same time. They are, therefore, in most instances found very near together while in the Fallopian tube; so that if a single ovum be obtained, it may be presumed that the rest are not far off. The Table (par. 319.) shows that I have often found ova at the commencement of the uterus, that is within an inch or half an inch of the Fallopian tube. They seem stationary in that locality for some time.

The size of the minute Ovum no criterion of the degree of its development.

167. The figures in Plates VI. and VII. illustrating "*stages*," I have thought it proper to draw on fixed scales, scrupulously copying nature in regard to size. If however, these figures be referred to, the not uninteresting fact will be made evident, that there is no fixed relation between the size of the entire ovum, and the degree of development of its most essential parts. An extraordinary instance of this is afforded in Plate VII. fig. 124. where in an ovum of less than $\frac{1}{3}'''$ the embryo had attained a stage far beyond what is usual in ova many times as large (par. 218.). On the other hand, Plate VI. fig. 110. presents an instance in which development (as compared for example with that of the ovum fig. 113.) appeared to have been retarded.

168. Nor do any two parts of the ovum necessarily keep pace with one another; a fact well shown in Plate VII. fig. 124., where the incipient umbilical vesicle ($bb^{2'}$) and the structure *am.* are very far behind the embryo in the degree of their development.

169. In the following description therefore of successive stages, it will not in general be desirable to state particularly the dimensions of the ovum, nor minutely to detail the condition of any of its parts but those that serve to mark the stage.

Third Stage of Development.

170. When discharged from the ovary in the state exhibited by Plate V. figs. 96. and 97., the ovum was found in the following condition at the distance of one inch from the infundibulum in the Fallopian tube (Plate VI. fig. 103. α.). The germinal vesicle (c), was visible in the ellipsoidal mass that occupied the centre of the fluid yelk. The latter (d) was obscurely granulous. The proper membrane of the yelk (e) was seen with remarkable distinctness, being indeed, from its appearance as a thick black line, the most conspicuous object in the ovum. The distention of the membrane f had proceeded farther, and in the same proportion a pellucid fluid had been imbibed. The tunica granulosa (g^1) was present, but the retinacula were not distinctly seen†. Ovum $\frac{1}{13}'''$.

171. At β (fig. 103.) the same ovum is shown ruptured, to demonstrate the strength

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† Traces of both the tunica granulosa and retinacula are, however, generally discernible, and often distinctly seen, at later periods.

of (*e*) the proper membrane of the yelk, which remained whole and still contained the yelk, though forced through the lacerated membrane *f*. The great thickness of the latter is seen in this figure; as well as the effect of pressure on (g^1) the half fluid tunica granulosa.

Fourth Stage of Development.

172. In Plate VI. fig. 104. α. is represented an ovum of forty-one hours, and measuring $\frac{1}{17}'''$, found about an inch from the infundibulum in the Fallopian tube. The tunica granulosa was not distinct. Around the thick, transparent membrane *f* was a dark circle (*cho*) which at first seemed the outer surface of that membrane, now exhibiting a high refracting power. The yelk (*d*) was obscurely granulous, and the germinal vesicle no longer seen†. At fig. 104. β. is another ovum of the same Rabbit found further advanced into the tube, and here represented after being crushed. This ovum presented the same dark circle (*cho.*) seen in fig. 104. α. On crushing the ovum, however, I found the dark circle to be *a thin membrane closely investing the thick transparent membrane f.* Pressure produced the following changes in that ovum; viz. The yelk-ball, *e*, (fig. 104. α.) became distended so as nearly to fill the membrane *f* (fig. 104. β.); the membranes *e* and *f*, then bursting, discharged their contents, which are seen lying between the membranes *f* and *cho.* No trace of fibres was observed in the membrane *cho.* On being crushed, it not only enlarged, but became elliptical, and bore very considerable pressure before being ruptured. This membrane (*cho.*) appears to be the chorion, which we shall subsequently find to become villous in the uterus (par. 222. 223.). It exhibits no small degree of elasticity.

Fifth Stage of Development.

173. The ovum seen in Plate VI. fig. 105., is one of 35¾ hours, and measured $\frac{1}{16}'''$. It was found near the middle of the tube. The thick transparent membrane *f* had more refracting power than in previous stages, and *asperities were observed on its outer surface*; that which previously seemed to constitute its external part having separated in the form of the membrane *cho.* described in the preceding stage‡. Instead of being invested, as in fig. 104. α., by a comparatively thin membrane, the membrane *f* was surrounded by a substance having a gelatinous appearance. The outer surface of this gelatinous looking substance, however, appeared to be constituted by the same

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† I have met with an ovum of twenty-three hours (in the Fallopian tube) differing from the one now under consideration, in there being within the yelk-ball (*e*) several large vesicles occupying the situation of the ellipsoidal mass in fig. 103. α., and surrounding a vesicle (germinal vesicle?) apparently having an opacity (germinal spot?) within it. These vesicles were contained in an obscurely granulous and fluid yelk.

‡ Plate IX. fig. 153. represents the mode of origin of the chorion. The ovum seen in this figure was one of seventeen hours, and found with five others near the middle of the Fallopian tube. It measured about $\frac{1}{17}'''$. The chorion (*cho.*) was rising from the thick transparent membrane ("zona pellucida") *f*, and surrounded by what remained of the granules (vesicles) of the tunica granulosa. The chorion was thus in a stage between the stages represented in Plate VI. figs. 104 and 105, while the interior of the yelk-ball (*e*) was less advanced than that in fig. 104. α., the germinal vesicle being still seen (par. 168.).

membrane (*cho.*) which in the last stage closely invested the membrane *f*. If an ovum of the present stage (fig. 105.) be crushed, the membrane *cho.* presents a sensible degree of thickness, and the fluid (f^1) lying between that membrane and the thick transparent membrane *f* is found to have no small consistence. The yelk of the ovum seen in fig. 105. did not present an appreciable difference from that in fig. 104.

Sixth Stage of Development.

174. An ovum of $\frac{1}{9}'''$ found with that just described (Plate VI. fig. 105.) is exhibited in fig. 106. The membranes *cho.* and *f* were in very nearly the same condition as those in the ovum fig. 105. The membrane *e*, however, of fig. 105. had disappeared (by liquefaction) in the ovum fig. 106, and there no longer existed a granulous yelk, as at *d* in fig. 105[†]. In the ovum fig. 106, the thick transparent membrane *f* ("zona pellucida") was filled with a transparent and colourless fluid (*d*) which it may be proper to designate the yelk. The centre of this fluid was occupied by four large vesicles. These vesicles were spherical, but somewhat flattened. They had a very high refracting power, and being exceedingly transparent, the contour of the remoter ones was distinctly visible through those nearer to the eye. Their contents appeared to be a fluid and granules. Some of these vesicles presented in their interior a minute pellucid space, which may possibly have been a nucleus[‡].

Seventh Stage of Development.

175. In an ovum somewhat larger, found in the Fallopian tube, a new set of vesicles (Plate VI. fig. 107.) had arisen, more numerous and smaller than the last, their appearance in other respects being the same. They also occupied a similar situation. The membranes of this ovum, not differing except in size from those of the ovum last described, have not been represented in the present figure.

Eighth Stage of Development.

176. In another ovum from the Fallopian tube, the vesicles in the centre of the ovum (Plate VI. fig. 108.) were found still more numerous and still smaller; in other respects not differing from those in figs. 106 and 107. The membranes of the ovum were in a state very closely resembling that of the corresponding parts in fig. 106.

177. The ovum probably passes through stages, which, in both the size and number of its central vesicles, are intermediate in reference to that I have endeavoured to re-

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[†] The term "yelk"—as applied to the contents of the ovarian vesicle of Baer—has not been discontinued in this memoir; but from the facts recorded in it—and more particularly from the changes delineated in Plate VI. figs. 105½. 106. 107 and 108.—I am disposed to question the analogy which this term implies (par. 122. and *Note*. 318.).

[‡] Later observations strengthen this supposition, and enable me to extend it to vesicles in the succeeding stages. The nucleus was very distinct in each of the two vesicles occupying the centre of the ovum in fig. 105½, a stage obviously between my "Fifth" and "Sixth," but not met with in time to be described in its proper place. The pellucid nucleus, however, in all of these vesicles, seems to be present during a certain period only of their existence (par. 315 to 317.).

present in fig. 108, and the condition to be next described. The figures given, however, will suffice to show the nature of the process, which may, perhaps, be analogous to that observed by **Pouchet**† in the ovum of a species of Limnæus. Possibly also, the divisions and subdivisions first noticed by **Prevost** and **Dumas** in the ovum of the Frog, and now known to occur in the ova of other Batrachian Reptiles, as well as in those of certain Fishes, may be referable to a process of the same kind‡ (par. 307. 318.).

Ninth Stage of Development.

178. An ovum of sixty-three hours, and of $\frac{1}{5}'''$, is represented in Plate VI. fig. 109. It was found in the Fallopian tube within about an inch of the uterus. The vesicles seen in the centre of the ovum—still minuter than those in fig. 108, and much more numerous—had ceased to be transparent, and were punctate from dark globules, apparently on their outer surface. The vesicles were nearly of equal size, and measured each about $\frac{1}{100}'''$. The structure formed by those vesicles presented a curious resemblance to a mulberry. In the ovum, fig. 109, this mulberry-like object had a diameter of $\frac{1}{15}'''$. The membrane *f* was irregular in its thickness, and at one part had become very thin (par. 190.). Plate VIII. fig. 128. represents the same ovum crushed. The chorion (*cho.*) became elliptical (pars. 172. 222.), a change in form not participated by the membrane *f*. The contents of the ruptured membrane *f*, proceeding no farther than the dotted line, showed this to be the inner surface of a thick membrane—the chorion—which (inner surface) had been concealed by an equal refracting power in the fluid f^1. The fluid f^1 in this instance had a tinge of yellow.

179. **Von Baer** found a little body in the uterus of the Dog, near the tube, which seems to have been an ovum in a state resembling that of the above. He describes it as consisting of a minute central sphere which was opake, with a transparent halo or periphery. The central sphere (my mulberry-like structure?) he conjectures may have been the vitellus or future intestinal sac, and the periphery he supposes was the "membrane corticale."

Tenth Stage of Development.

180. Seven ova, one of which is exhibited in Plate VI. fig. 110, were found in the Fallopian tube within about three quarters of an inch of the uterus. The mulberry-like structure measured $\frac{1}{10}'''$, presented a greater number of vesicles—which were still somewhat smaller than the last—and the interior of its vesicles was more distinct. Their increased transparency seemed partly referable to the absence of dark globules seen in the preceding stage. Having a high refracting power, their outline was extremely well defined and sharp; more so than I have been able to represent it with a pencil. Within each vesicle was seen an object§ resembling the "germinal vesicle-

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† Froriep's Notizen, No. 138, Julii 1838.

‡ **Schwann** has suggested that the divisions in question in the ovum of the Frog, may, perhaps, be reducible to a "cell"-formation (*l. c.* pp. 61, 62.).

§ In one instance two of such objects were observed in the same vesicle (par. 317. *Note.*).

like nucleus" observed by Valentin† in "globules" from various parts of the nervous system. This object was round, colourless, and pellucid, and contained a central dark point, resembling the "corpuscle" of the above author. The chorion (*cho.*) was distinguishable from the fluid f^1. Plate VIII. fig. 130, exhibits another of the seven ova crushed. The membrane f is here seen distended and ruptured under the compressor. The mulberry-like object—when crushed—not only filled the cavity of that membrane, but a part of it escaped, and its vesicles became pressed together into figures of several sides. The nuclei remained unaltered: their dark central points (nucleoli), however, in this instance had the appearance of globules. In the space between each vesicle and its nucleus, and chiefly *around the latter*, were now seen granules. Many of those granules, escaped from ruptured vesicles, are represented in the figure, lying in contact with the chorion (*cho.*).

The importance of examining Ova from the Fallopian Tube.

181. Cruikshank‡ found ova of the Rabbit in the tube, and we are indebted to him for very important information regarding their minuteness; but the microscope in his day was not in a state to admit of his seeing their internal structure. His figures therefore are mere specks.

182. There exists another representation of an ovum of the Rabbit (and I believe only one) taken from the Fallopian tube. It is contained in a paper by T. Wharton Jones§. That ovum was one of "the third day," found with five others in the tube "near where it enters the horn of the uterus," and in size ("$\frac{1}{70}$th of an inch") appears to have been between those which I have represented in Plate VI. figs. 109 and 110. My observations corroborate those of the author just mentioned in reference to the *appearance* of the envelope in ova of this period, but they do not agree with his views as to its real nature. T. Wharton Jones‖ describes this envelope as "a thick gelatinous matter." In all the ova I examined, the outer portion of the envelope was already in the condition of a formed membrane, which condition it had from the first retained (Plate VIII. fig. 128., Plate VI. fig. 110. *cho.*). (The previous existence and mode of origin of that membrane I have already shown in Plate VI. fig. 104. α. and β. and in Plate IX. fig. 153.). In reference to the interior of ova of this period, my observations do not enable me to corroborate those of Jones; who remarks¶ "the granulary matter of the yelk was coherent." (Contrast with Plate VI. fig. 105½ to 110.)

183. Prevost and Dumas†† found no ova of either Dogs or Rabbits in the Fallopian tube, and the smallest ova they saw in the uterus measured $\frac{1}{2}'''$. Coste‡‡ has not figured an ovum from the tube in any animal. This author remarks§§, "after conception, we have stated, the vesicle which we know to be the analogue of the vesicle of

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† Ueber den Verlauf und die letzten Enden der Nerven, figs. 51, 52, 70. 1836.

‡ *L. c.*

§ Philosophical Transactions, 1837, Part II. plate xvi. fig. 1.

‖ *L. c.* p. 339.

¶ *L. c.* p. 339.

†† *L. c.*

‡‡ Embryogénie Comparée.

§§ pp. 109, 110.

Purkinje, dissolves, and the ovum *then* presents itself under the aspect of a *crystalline vesicle perfectly homogeneous*. The space which was occupied by the yelk, the condensation of which has served to form the blastoderma, is filled with a transparent fluid." With this statement I would contrast nine stages of the ovum of the Rabbit from the Fallopian tube (Plate VI. figs. 103—110). Von Baer† has figured one ovum from the Dog, found in the Fallopian tube. It would not, perhaps, be fair to contrast this with ova from the Rabbit; but the Professor certainly came prematurely to the conclusion that "in their passage through the tube the ova of Mammalia undergo scarcely any metamorphosis at all." I refer to Plate VI. figs. 103—110. in proof that there is at least one of the Mammalia to which this statement is inapplicable. Does the Dog differ so widely from the Rabbit, that in the tube its ova undergo scarcely any change?

Eleventh Stage of Development.

184. A layer of vesicles (Plate VI. fig. 111.), in all respects of the same kind as those constituting the mulberry-like appearance before mentioned, had now been added. This layer—resembling an epithelium—lined the membrane *f*, which it will be remembered had been the thick transparent membrane, or "zona pellucida" of the ovarian ovum. The mulberry was still in the *centre* of the yelk.

Twelfth Stage of Development.

185. The mulberry-like structure (Plate VI. fig. 112.) was on its way from the centre to the surface of the yelk. (The chorion (*cho.*) in this instance was distinguishable from the fluid *f'*.)

Thirteenth Stage of Development.—The true Germ.—The so-called "Serous Lamina of the Germinal Membrane" a Structure of subordinate importance.

186. In this stage the mulberry-like structure (Plate VI. fig. 113.) has reached the surface of the yelk; its own vesicles on one side, as well as some of those of the peripheral layer, have disappeared, and a vesicle contained in the mulberry-like structure comes into view. This vesicle lies in close contact with the membrane *f*. It is flaccid, and in its present situation appears flattened and elliptical. It contains a fluid and dark granules, and highly refracts light, which seems partly owing to the presence of those granules in considerable quantity on its inner surface. In two instances this vesicle measured in its long diameter about $\frac{1}{36}'''$; in another instance rather less. In the centre of the fluid of this vesicle is a spherical body (*bb*), yellowish brown in colour, and composed of a substance having a finely granulous appearance, which is distinctly circumscribed. It has a cavity in its centre containing a colourless and brightly pellucid fluid. This hollow spherical body seems to be the true germ. It measured in the present instance (fig. 113.) $\frac{1}{77}'''$; its central cavity

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† Lettre, &c., fig. 3. III. and III*.

less than $\frac{1}{300}'''$. I have seen the stage now before us in a considerable number of ova measuring $\frac{1}{8}'''$ and $\frac{1}{7}'''$†.

187. Authors on the ovum of the Bird describe their "primitive trace" of the embryo as originating in that which has been denominated the "central, thickened part of the germinal membrane." In the mammiferous ovum now under consideration (fig. 113.) the vesicles lining the membrane *f* appear to me to represent the so-called "germinal membrane" (or what has been denominated its "serous lamina"), and the remains of the mulberry-like structure seem to correspond to that which has been considered its "central thickened part." If, however, facts to be hereafter stated should render it very probable that the foundation of the new being is that contained within the mulberry-like object, figs. 109 to 113, it will perhaps appear that this foundation is no part of any membrane. The layer of vesicles (*am.*) lining the membrane *f* (fig. 113.), with those previously constituting the external part of the mulberry-like object, we shall find to form a structure of subordinate importance—the amnion (par. 199. 200.)

Fourteenth Stage of Development.—The Area pellucida.

189. The vesicle described as contained in the mulberry-like structure, and as coming into view in the preceding stage, was no longer to be discerned in the ovum represented in Plate VI. fig. 114. In its place there was an elliptical depression (*a. p.*), filled with a colourless pellucid fluid, presenting an indistinctly granulous appearance at its margin, and containing in its centre the germ (*bb*), which had nearly the same appearance as that in fig. 113, but was somewhat larger. The elliptical depression here mentioned appears to correspond to the *area pellucida* of authors on the Bird.

Fifteenth Stage of Development.

190. The vesicles forming the outer part of the mulberry-like structure had coalesced, where in contact, with those of the layer (Plate VI. fig. 115. *am.*) lining the membrane *f*; but they still formed a projection. The membrane *f* was attenuated (par. 178.), and projected in some degree at the part where the germ was observed to lie. (In another ovum found at the same time, slight pressure caused a sort of hernia at this part, one of the vesicles in the layer *am.* protruding through the membrane *f*. This attenuation of the membrane *f* may perhaps have reference to imbibition.) The vesicles *am.* appeared enlarged or flattened, and pressed together into polyhedral forms (Plate VIII. fig. 129.), and dark globules were seen at their periphery. The interior of those vesicles was very distinct; and if the figure now referred to be com-

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† Perhaps it would be more correct to consider the vesicle itself (which forms the interior of the mulberry-like structure) with the whole of its contents as the true germ. The particular period at which the formation of the germ (as such)—within the mulberry-like object—commences, my observations do not enable me to state; but in a stage apparently rather more advanced than that represented in Plate VI. fig. 108, I have seen, on the application of gentle pressure, that the mass of spherical objects, occupying the centre of the ovum, contained a fluid in its interior.

pared with Plate VIII. fig. 130 (which represents an ovum of the Tenth Stage), there will be found this difference; that in the space between the membrane of each vesicle and its nucleus there are seen in the present instance (fig. 129.) a number of dark globules, not present as such in the earlier stage (par. 180.).

191. Von Baer appears from his description† to have met with ova of the Dog in either this or a neighbouring stage; though certainly no drawing given by that eminent observer enables me to recognise the resemblance. He mentions having observed a mass of granules, which was conical in the minuter ova, and in those more advanced discoid in its form.

Sixteenth Stage of Development.

192. The layer of vesicles *am.* in the ovum Plate VI. fig. 116, appeared to be passing into the condition of a membrane. Those parts of that layer which formed the *sides* of the area pellucida (*a. p.*) were raised and approaching one another, while at the ends of that pellucid space there was no such tendency, but, on the contrary, the appearance of depression into a sort of channel. The sides, here seen to have been in near approximation, appear subsequently to come into contact and unite (Plate VII. fig. 121 D.). (If this supposition be correct, Plate VII. fig. 122—representing a much later stage—shows this union to have taken place at one point; and in figs. 121 A. and 121 B. the union has been more extended. In fig. 122. there is seen a circular space on each side of the point of union. These circular spaces seem to represent the parts which in the sixteenth stage had the appearance of being depressed into a sort of channel, or in other words, exhibited no tendency to become raised over the area pellucida).

Seventeenth Stage of Development.—Central and Peripheral Portions of the Germ.—Origin of the Lamina subsequently vascular.

193. About this time the germ separates into a central and a peripheral portion. In the figure representing the present stage (Plate VI. fig. 117.) the ovum is seen in profile, and the germ therefore is not visible. (An idea may be formed of the separation here mentioned from Plate VIII. fig. 148, in which bb^1 is the central, and bb^2 the peripheral portion of the germ‡.)

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† Lettre, &c., p. 12.

‡ The germ is here represented in its vesicle as seen while still in the centre of the ovum. (The mulberry-like structure was imperfect, and hence the possibility of seeing the objects in its interior.) In this instance the incipient separation of the germ into a central and a peripheral portion appeared to have been premature. These central and peripheral portions of the germ are represented in Plate VII. figs. 121 A. 121 B. and 122. bb^1 and bb^2. Whether they really arise from a separation of the object *bb* (Plate VI. fig. 113 to 116.) into two portions, future observation must decide. Possibly the object *bb* disappears by liquefaction, and a linear trace, corresponding to the "primitive trace" of authors on the Bird, arises in its place. In either case, however, the terms *central* and *peripheral portion of the germ* will be useful in the present memoir, and in either case the embryo does not arise in the substance of a membrane.

194. Before mentioning the most remarkable feature in this ovum, I would refer to the opinion generally received at present regarding the manner of origin of the "mucous lamina" of the so-called "germinal membrane." This, however, has hitherto been the subject of conjecture only, as will be amply shown by the following extract, containing perhaps the latest that has been written on the subject; and it comes from a very high authority, that of Rathke †.

"Pander, Baer, and I,—the former in reference to the Chick, I in reference to the Crafish,—have used the expression that it [the mucous lamina] separates by splitting from the serous. We thus gave it as our opinion, that this process consisted in the splitting of the germinal membrane. More recently Baumgärtner has expressed doubts of such an origin of the mucous lamina, and advanced the opinion that it becomes deposited upon the serous lamina, by the latter exercising an attractive influence upon the yelk, which determines single parts of the same to arrange themselves densely on it (the serous lamina) to form a new structure [the mucous lamina]." The Professor, after stating his objections to this opinion of Baumgärtner, says there are only two ways conceivable in which the mucous lamina can arise, viz. it is either thrown off by the serous lamina, or originally there exists a single mass which splits into the serous and mucous laminæ. In order to obtain a solution of this question, he again examined the ovum of the Crafish; but as that did not satisfactorily furnish it, he concludes that it would be advisable again to examine the ovum of the Bird.

195. We thus see that there is still great uncertainty as to the manner of origin of the "mucous lamina" of the so-called "germinal membrane." I have no speculations to offer on the subject, and shall do little more than refer to figures in which it has been attempted to represent nature in ova of one of the Mammalia.

196. From the region occupied by the germ, there extended in the ovum representing the present stage (Plate VI. fig. 117.) a hollow process ($bb^{2\prime}$) consisting of *exceedingly pellucid* objects, which hung loosely together, and were somewhat depressed where in contact. This process seemed to pass through the central part of the now flattened mulberry-like structure, before described, and to be connected with the germ. In what manner it was so connected my observations on that ovum do not enable me with certainty to state; but later stages show it (the process in question) to enter into the formation of a structure ($bb^{2\prime}$) *continuous with* that which I have called the peripheral portion of the germ (bb^2 in several figures of Plate VII.). In later stages this hollow process attains a size sufficient to line the cavity, the centre of which it occupied in the ovum, fig. 117; and probably in proportion as this process widens at its origin, the remains of the former mulberry-like structure disappear. Provisionally I may perhaps be permitted to consider the process in question as an incipient state of the umbilical vesicle. Should it prove to be so, its mode of origin must be very different from that which has hitherto seemed the most probable to authors on the ovum of the Bird (par. 194,). The pellucid objects which hang together and constitute

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† Zur Morphologie Reisebemerkungen aus Taurien, p. 104, 1837.

the principal part of the process in question, appear, however, to be the foundation of, not the *mucous*, but the *vascular* lamina of the umbilical vesicle; which latter, therefore, according to my observations, is the first of those two laminæ coming into view. The observations by RATHKE[†] on the Crafish, above referred to, seem to me to contain internal evidence corroborating the description I have just given. Thus he mentions a layer of minute albuminous granules as present on the inner or yelk surface of the germ, soon after it has arisen, these granules being in some parts very loosely connected among themselves. Still more in accordance with my observations on the origin of the umbilical vesicle in Mammalia, are those of the same eminent author in a former series of Researches[‡], on the first trace of the subsequent posterior half (that is the abdomen) of the Crafish. This structure, according to RATHKE, presents itself as a *little sac, finely granulous in its substance, issuing from the bottom of a depression existing at the surface of the yelk.*

Eighteenth Stage of Development.—First Change in Form presented by the Germ.

197. In Plate VII. fig. 118. bb^1. the central portion of the germ presents a pointed process (par. 213.). In previous stages the germ had a finely granulous appearance, and was comparatively pale in colour. In the ovum now before us (fig. 118.) the central portion of the germ was nearly black, apparently from globules of extreme minuteness. It seemed distinctly circumscribed, and contained a pellucid cavity in its larger end. It measured in length $\frac{1}{30}'''$. (The embryo in its most incipient state is subject to considerable variation in both its form and the appearance of the globules of which it is composed. Of this an instance is afforded in fig. 121 D.)[§]

198. A dark object, represented in the middle of the same figure (fig. 118.), appeared to line part of the inner surface of the ovum. That object was so obscured by blackish globules that I remain in entire ignorance of its structure; and having seen but a single ovum which in reference to the object in question was in that particular condition, I am equally incapable of stating in what manner it arose. The process ($bb^{2\prime}$) described in the preceding stage was discernible, and the dark object in the present figure may possibly have been situated in the interior of that process. We shall find apparently the same object to present itself in later stages (par. 203—206.).

199. The stratum of vesicles *am.* (fig. 118.) had passed into the condition of a membrane, and the space containing the germ had become more defined. The cause of the latter change appears to be the following; which, however, is offered as no more than probable, as my observations do not extend to a period sufficiently advanced to admit of certainty. That portion of the membrane *am.* (see the figure, and more particularly in later stages, figs. 121 A, 121 B, and 122) which surrounds the germ sinks

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[†] Zur Morphologie, &c. p. 106.

[‡] Ueber die Bildung und Entwickelung des Flusskrebses, pp. 12, 13.

[§] See that portion of the Note to par. 193. which relates to the possibility of the object *bb* (Plate VI. fig. 113 to 116.) disappearing by liquefaction.

in, while the externally adjacent portion of the same membrane (*am.*) is raised,—the part raised being double. The sinking in of the membrane *am.* around the germ, I suppose to indicate the commencement of a rising or separation of the latter from the surface of the yelk; and the elevation of the adjacent and external portion of the same membrane (*am.*) appears to me to denote the incipient formation of the amnion. To those who have investigated the development of the Bird, this will be familiar. For the information of others it may be added, that (if the explanation given by authors on the ovum of Birds be applicable to that of Mammals) the double membrane *am.*—by continued elevation—is made to arch over the embryo, and finally to meet and join. The outer lamina (of the fold of membrane raised) is then thrown off, while the inner lamina constitutes the amnion. I refer to the plates of Baer[†], showing the mode of formation of the amnion in the Bird.

200. In adopting, however, the explanation which has been given of the manner of formation of the amnion in the Bird, I must be understood as maintaining—in opposition to the views of others—that the membrane so appropriated in Mammalia is no part of that structure out of which the embryo is formed (par. 187.). The membrane now referred to as forming the amnion, is that marked *am.* in Plate VI. figs. 113—117. It consists of the epithelium-like layer of vesicles (fig. 111.) on the inner surface of the membrane *f*, to which the vesicles presenting the appearance of a mulberry are subsequently added (fig. 113.), and with which they coalesce (fig. 115.) to form the membrane *am.* in later stages (see Plate VII.).

Nineteenth Stage of Development.—Hollow Network in the Ovum.

201. The process (Plate VII. fig. 119. $bb^{2'}$.) first mentioned in the seventeenth stage (Plate VI. fig. 117.), as consisting chiefly of pellucid objects hanging loosely together, has now enlarged so as to apply itself to all parts of the inner surface of the membrane which in other figures has been marked *am.* It now constitutes a membranous hollow network[‡]. In Plate VIII. fig. 132 is exhibited a portion of this network highly magnified. It presents elliptical enlargements, containing a yellowish turbid fluid, and a nucleus which is spherical, colourless, and *remarkably pellucid.* Around each of these nuclei are dark globules. The pellucid objects entering into the formation of the process $bb^{2'}$ in Plate VI. fig. 117 appear to have been incipient vesicles just rising from their nuclei. It is probable, that subsequently those vesicles distend, and at the parts where they are in contact with one another, coalesce in such a manner as to make their cavities continuous. In this way the structure by distention may form the hollow network just described[§].

[†] Ueber Entwickelungsgeschichte der Thiere. Beobachtung und Reflexion. Erster Theil, tab. ii.; also Burdach's Physiologie, vol. II. tab. iii.

[‡] This ovum measured $\frac{1}{4}'''$ +. I have met with ova of $2'''$ apparently not more advanced in reference to the network (par. 167. 168.).

[§] We shall hereafter find this explanation to be in accordance with Dr. Schwann's view of the mode of origin of capillary vessels (par. 295.), though it is another structure which is here produced.

Twentieth Stage of Development.

202. The network has disappeared (Plate VII. fig. 120.), but the nuclei (*bb*$^{2'}$) which were contained in its enlargements remain. The membrane of the network appears to have liquefied, and furrows filled with fluid mark its former situation. Some of this fluid surrounding the nuclei, points out the place previously occupied by the enlargements in the network. The nuclei have still the peripheral accumulations of dark globules, which existed while they were contained in the network.

203. The nuclei are situated on a lamina internal to them. This lamina may, perhaps, be the dark object mentioned in the "eighteenth stage," which has now enlarged so as to contain the yelk. Whether this lamina is the foundation of the mucous lamina, or whether it contributes to the formation of the lamina subsequently vascular, or to that of both, my observations do not enable me to state.

Twenty-first Stage of Development.

204. The furrows visible in the twentieth stage have disappeared; but the pellucid nuclei remain (Plate VII. fig. 121. *bb*$^{2'}$.), and are still surrounded by dark globules[†].

Multiplicity of parts in a minute Ovum.

205. The ovum from which fig. 121. was taken measured $\frac{2}{5}'''$. A drawing of that ovum occupies the centre of Plate VII. (fig. 121 A.). I do not suppose, that with the condition of the future umbilical vesicle exhibited in fig. 121—and forming my twenty-first stage—the state of the whole ovum is always such as that in fig. 121 A,—because as already said (par. 168.) the parts do not necessarily keep pace with one another[‡]. It may, however, be desirable to mention the structures of which that ovum was composed. (In fig. 121 A. it presents the appearance of *incipient* collapse; this having been the effect of the fluid—kreosote water (par. 239.)—in which it lay when drawn.)

206. Proceeding from the exterior inwards, we find the parts of the ovum in question to be as follows; viz. *cho.* is the chorion; f^1, fluid; *d.* yelk which has escaped from its cavity, and not mixed with the fluid f^1; *f*, the thick, transparent membrane of the ovarian ovum ("zona pellucida"); *am.* the amnion; *am.f*, a part at which the membrane *am.* now adheres to the membrane *f*; *bb*^{1}, central, and *bb*^{2} peripheral portion of the germ. Continuous with the peripheral portion of the germ is the subsequently vascular lamina of the umbilical vesicle, *bb*$^{2'}$, having a lamina internal to it. Within the part last mentioned is the yelk.

207. Thus the ovum of the Rabbit may pass through at least one-and-twenty stages of development, and—as in the ovum just described—may contain, besides the

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[†] VON BAER has figured objects seen in an ovum of the Dog, which appear to have corresponded to the nuclei and dark globules above-mentioned. (Lettre, &c. p. 12. fig. V*.) His description of them, however, does not at all accord with my observations on ova of the Rabbit.

[‡] And I have met with ova many times as large which were not more advanced in their development.

embryo, four membranes, one of which has two laminæ, before it has itself attained the diameter of half a line; one membrane moreover (denoted by the letter e in some of the figures of Plates V. and VI.) having disappeared by liquefaction in the Fallopian tube. Hence the importance of examining ova when minute. The smallest ova found by Prevost and Dumas in the Dog, measured half a line,—that is, rather more than the ovum I have now described. But the ova met with by those observers seemed to them to consist of *a single membrane*†. Von Baer‡ mentions ova from the Dog of the same size ($\frac{1}{2}'''$) as composed of *two membranes*, the inner having granules on its internal surface (par. 204. note). It is, however, only fair to add, that the size of minute ova affords no criterion of the degree of their development,—and also that in this respect there may be a difference in different animals; though these considerations are scarcely sufficient to explain the absence of two or three membranes. The membrane *f*—unless its presence has been ascertained from the examination of very minute ova—may easily escape notice as a separate structure in ova more advanced§.

Adhesion between the thick transparent Membrane of the ovarian Ovum and the Membrane which forms the Amnion.

208. It has just been stated that the membrane *am.* adheres at a certain part to the membrane *f* (Plate VII. fig. 121 A. *am.f.*). I have observed, that the points adhering do not constitute a complete circle or ellipsis, but are interrupted at that part which is in the neighbourhood of the caudal extremity of the embryo. Here the adhesion (at least originally) does not take place. This adhesion appears to correspond to one known to occur in Birds; and possibly it takes place in the Mammal for the same purpose as that which it is supposed to answer in the Bird, viz. to promote the

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† *L. c.* No. 188, p. 182. Prevost and Dumas state indeed that larger ova of the Dog, viz. $\frac{3}{4}'''$ to $1'''$, consisted of a single membrane.

‡ Lettre, &c. pp. 11, 12.

§ T. Wharton Jones (*l. c.*, p. 341. and fig. 6.) gives the following description of two ova found in the horn of the uterus of the Rabbit seven days after impregnation, and measuring about $\frac{1}{30}$th of an inch (between $\frac{1}{4}$ and $\frac{1}{3}$ of a French line) in diameter. "No vitellary membrane was to be seen. The gelatinous-looking envelope constituted the only covering of the yelk, which now formed a vesicular blastoderma. The cavity of the gelatinous-looking envelope was much larger than the vesicular blastoderma. The inner surface of the gelatinous coat presented what I supposed to be fragments of the vitellary membrane adhering to it. In both ova the vesicular blastoderma was irregular on one side, that on which I supposed the embryo was about to be developed. It was beginning to present the separation into layers, and had the same peculiar friable globular structure as the blastoderma of the Hen's egg." In reference to this description, I am compelled to state that Plate VIII. fig. 138. represents the vitellary membrane (*f*) entire, as seen by me in an ovum of $\frac{2}{10}'''$; that I have not met with any such phenomenon as the separation of a membrane into layers; and that the result of my observations on the subject of a "vesicular blastoderma"—having a peculiar "friable globular structure"—may be found in Plate VI. figs. 105½ to 117., and in Plate VII. figs. 118 and 119. *am. bb.* and $bb^{2'}$; also in par. 187. 192. 199. 200., and par. 196. 198. 201. The real nature of the "gelatinous-looking envelope" has been already explained (par. 172 to 174. 178. 180. 182.).

rising of the membrane *am.* for the formation of the amnion (par. 199.). In Plate VIII. fig. 145. is exhibited an ovum drawn after it had lain six weeks in dilute spirit. The membrane *am.* (with its contents) is here seen to have been pendent from the membrane *f*, through the adhesion now mentioned. Possibly this adhesion may assist to explain why the incipient embryo which it incloses, is (as I have found it) generally either uppermost or undermost when the ovum is viewed in a fluid medium. The relations of the thick transparent membrane ("zona pellucida") of the ovarian ovum in stages subsequent to that which I have called the "fifth" (Plate VI. fig. 105.), and the adhesion just described as taking place between this membrane and the membrane entering into the formation of the amnion, may, perhaps, be considered as showing the correctness of those who had conjectured the thick transparent membrane to be analogous to the vitellary membrane in the ovum of the Bird (par. 174.).

The Embryo a Congeries of Vesicles.

209. The precise condition of the embryo has not been mentioned in the three last stages; the fact being that its appearance undergoes such rapid changes, and is subject to such variation, that to have attempted to associate any particular condition of it with that of the parts representing those stages, would have been quite fruitless, and moreover calculated to mislead. Besides which, there are so many dark globules mixed with the vesicles, of which the embryo is now composed, that it is extremely difficult to ascertain what the condition of the latter really is;—a difficulty augmented by the tendency in many instances to a sinking in at that part where the embryo lies. If however fig. 118. be compared with fig. 121 A., the following differences will be observed. In the former, the central portion of the germ (*bb*^{1}) presented globules of extreme minuteness; in the latter it was a congeries of distinct vesicles, of which moreover there were two states. Those most internal were smaller and appeared nearly black; while those of the outer set were more expanded, and paler in their colour. The peripheral portion of the germ in fig. 121 A. (*bb*^{2}) was seen with great distinctness; and between the central and peripheral portions of the germ there were extended cords formed of vesicles, having the appearance and apparently performing the office of retinacula.

Stages of Development later than the Twenty-first.

210. If I have succeeded in making plain the foregoing "stages," later ones will not require to be described so much in detail. Nor is it my purpose to extend the present paper to stages in continuous succession, beyond that which I have called the twenty-first.

Progressive Formation of the Vascular Lamina of the Umbilical Vesicle.

211. The subsequently vascular lamina of the umbilical vesicle in the twenty-first stage (Plate VII. fig. 121. *bb*$^{2'}$.) consisted of scattered nuclei, having peripheral accu-

mulations of dark globules. In a stage somewhat more advanced, though in an ovum of about the same size (Plate VIII. fig. 150.), there were seen, not scattered nuclei, but vesicles pressed together into a polyhedral form, each vesicle containing its colourless and pellucid nucleus. Some of the nuclei contained a dark globule; and in the vesicles were globules, situated especially *on the nuclei* (par. 304.). As already stated, it is probably in this lamina that blood-vessels subsequently form[†].

Arrangement of the Vesicles composing the Embryo, and Order of their coming into view as Vesicles.

212. A condition of the germ or embryo seen in Plate VII. fig. 121 B, appears to represent the state succeeding that exhibited in fig. 121 A; and both of these figures, it may be added, were taken from ova of the same Rabbit. The germ in fig. 121 A. has been already briefly referred to (par. 209.). If fig. 121 B. be compared with it, the following differences will be observed. In the earlier state (fig. 121 A.) the peripheral portion of the germ (bb^2) was cordate,—in the latter (fig. 121 B.) it was somewhat lyrate in its form. (In this respect, however, I have observed some variation.) In the less advanced ovum (fig. 121 A.) the central portion of the germ (bb^1) appeared to consist of two parts, an internal and an external,—while in that more advanced (fig. 121 B.) it consisted of three distinct parts,—an internal, a middle, and an external. Thus in the later stage a new part had come into view. The new part seemed to be that which occupied the most central situation, parts previously situated there having been pushed farther out. Each of the several parts or layers now referred to was so distinctly circumscribed, as to appear almost membranous at its surface.

213. More particularly compared, the two figures in question exhibit farther differences. In the less developed ovum (fig. 121 A.) the most internal object was a dark trace, enlarged and hollow at one end, pointed at the other. In the ovum more developed (fig. 121 B.) the corresponding part occupied, not the most internal, but the second or middle place. Instead of being hollow merely at one extremity, it was now a hollow tube, with an enlargement at both ends. The part which had subsequently come into view (fig. 121 B.) was at the cephalic end. This part is shown more highly magnified in fig. 121 C. It consisted of two portions, one of which was spherical, and the other seemed a process from the first. The spherical portion contained a cavity filled with a brightly pellucid fluid. The external surface of this object was distinctly circumscribed, and almost membranous. Its substance appeared granulous at some parts, and at others presented globules or incipient vesicles. At a certain part the formation of globules (vesicles) had proceeded so far as to constitute the process above mentioned; and over this process the outer membrane (if such it may be called) was continued. (This object and the changes now described will perhaps serve to convey

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[†] Whether the lamina represented in Plate VII. fig. 121—as having the scattered nuclei lying on it—enters into the formation of the vesicles in Plate VIII. fig. 150, I am unable to determine.

an idea of the manner in which the central portion of the germ (figs. 118, 121 D. bb^1) undergoes its *first* change in form, already pointed out (par. 197.))[†].

214. In fig. 122. is seen a more advanced condition of the same parts. The minute body corresponding to that just referred to in fig. 121 C. had assumed in the embryo fig. 122. a different form (resembling that of the most central part in fig. 121 A.), and in a still later stage (fig. 123.) the corresponding part had in its turn become a tube, having very much the form of that which in the embryo fig. 121 B. occupied the second or middle place[‡].

215. The parts are dark in proportion as they lie near to the centre of the germ, which seems owing to the less expanded state of the vesicles of those parts, and to myriads of others which are coming into view.

216. In fig. 122. is a band of vesicles forming a sort of arch. The absence of the membrane *am.* at certain parts has been already mentioned (par. 192.), in reference to this figure as well as others. One object of the open spaces thus occasioned, may possibly be to admit of certain parts of the germ or embryo continuing in more immediate communication with the exterior than would have been the case had a membrane intervened (par. 190.); and coincident with the existence of those spaces is the fact, that the vesicles forming the peripheral portion of the germ do not make their appearance there in the same quantity as elsewhere,—while the accumulation of those vesicles at the parts over which the membrane *am.* does extend, presents the band or arch in question.—The peripheral portion of the germ (including the band or arch just mentioned), as already stated, is continuous with the subsequently vascular lamina of the umbilical vesicle. In later stages, as the central portion of the germ advances in its size, the arch in question seems to undergo a change in its situation, and to become relatively very small (par. 219. and Plate VII. fig. 124. bb^2.). Is not the peripheral portion of the germ the foundation of the heart and great vessels? (Compare, for instance, the arch above referred to in Plate VII. fig. 122. with representations by Professor SCHULTZ[§] of the origin of those parts in the Bird.) If so, my "peripheral portion of the germ" obviously corresponds to the "area vasculosa" of authors on the ovum of the Bird[‖].

Foundation of the Central Portion of the Nervous System and of the Vertebræ.

217. In Plate VII. fig. 127. I have attempted to exhibit the visceral surface of the future central portion of the nervous system and the incipient vertebræ, in an embryo

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[†] See the Note to par. 193.

[‡] In these figures the enlargement at the lower extremity indicates the situation of the future sinus rhomboidalis. It is not intended in par. 212 to 215. to be implied that no additions of vesicles are made externally. The nature of these, however, my observations do not enable me to state.

[§] Das System der Circulation in seiner Entwickelung, Tab. V. and VI. Stuttgart and Tübingen, 1836.

[‖] PREVOST and DUMAS appear to have seen an ovum of the Rabbit in a state between that exhibited in my figs. 123 and 127. (*l. c.* fig. 13.). No observation of mine leads me to suppose with PREVOST and DUMAS that the Spermatozoa enter into the formation of the central portion of the nervous system; though as to the early appearance of this part, my observations to a certain extent agree with theirs (par. 312.).

which measured rather less than $1\frac{1}{2}'''$ in length. The ovum containing it was fixed in the uterus, and had a diameter of more than six lines. The period was $8\frac{1}{2}$ days. This embryo was brought into view by dilute nitric acid. The foundation of the spinal chord and its sinus rhomboidalis are visible between the two rows of incipient vertebræ. **Cruikshank**[†] appears to have seen the embryo of the same animal (the Rabbit) in a condition resembling this.

An Embryo of remarkable Minuteness.—Effects of Flexion of the Embryo.

218. I have stated that within certain limits the size of the entire ovum affords no criterion of the degree of advancement of its parts. In only a single instance, however, have I met with so remarkable a proof of this as is afforded in Plate VII. fig. 124, where in an ovum of less than one third of a line, the embryo had attained a stage in its development approaching to that in another instance in which it measured many times the length. In fig. 124. the embryo was only $\frac{1}{4}'''$—or about $\frac{1}{45}$th of an English inch—in length; being thus little more than half that of the object represented in fig. 122, though the latter was beyond all comparison behind it in the degree of its development. So remarkable a deviation in point of size is probably of rare occurrence.

219. The embryo of the ovum, fig. 124, is shown more highly magnified in figs. 125 and 126. As viewed on one side it might have been compared to a sort of spoon; but on the spinal surface (of which, however, I could not obtain a direct view) it seemed in part unclosed. It was opake, had a granulous appearance, and was yellowish-brown in colour. When first seen the cephalic extremity was somewhat bent upon itself, as represented in fig. 125; and at a certain part the margin had become wrinkled. On slight pressure being applied, this extremity was observed to fall back into the nearly straight condition exhibited in fig. 126, when the wrinkles were no longer seen. The connexion of this wrinkled appearance with flexion of the embryo is interesting. The embryo had the form of a marrow-spoon. Flexion of such an object upon its hollow surface produced wrinkles at the margin. With continued flexion the wrinkles would doubtless have passed into folds. It hence appears that some of the earliest divisions of the embryo into more special forms[‡], are effected by the flexion on itself above described[§]. The fluid in which the embryo was contained (fig. 124.) appeared somewhat gelatinous, and was in no small degree transparent, though in many parts obscured by dark globules. It seemed to be invested by a de-

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[†] *L. c.*, Tab. IV.

[‡] See figures of the embryo of the Common Fowl in **Huschke's** paper on the first development of the eye. **Meckel's** Archiv, 1832. Sechste-Band.

[§] Several dark globules noticed lying together at one point, perhaps indicated the incipient formation of the eye. Unfortunately the drawings are in little more than outline. The sketch, fig. 126, was not commenced until I had examined the object for a considerable time, so as to be quite sure that I understood it in all its parts; and then on beginning to draw it, I had produced only what is shown in fig. 126, when the ovum became shrunk, somewhat dried, and so much altered, that I did not venture to proceed, and no addition has been made to that drawing since.

licate membrane, which I conjecture was a part of, and continuous with, the layer of vesicles (*am.*) lining the membrane *f*[†]. This layer in other parts, it will be observed, was very far behind in the degree of its development (par. 168.). Such was the case also with the process forming, as I suppose, the incipient vascular lamina of the umbilical vesicle $bb^{2\prime}$. The situation of the latter structure was such as to confirm my views as to its place of origin. A comparatively opake object (bb^2) crossed the embryo near its middle, and prevented my seeing the latter distinctly at that part, which therefore has been represented by dotted lines. I am disposed to think that this object corresponded to the arch (bb^2) represented in fig. 122. and if so, what has been already stated (par. 216.) on the continuity of that arch with the subsequently vascular lamina of the umbilical vesicle, will be applicable here. Farther, the object bb^2 in fig. 124. occupied a situation not very remote from that of the future great blood-vessels and the heart[‡].

220. The continuity already pointed out (pars. 196. 201—206. 216. 219.) between the peripheral portion of the germ (Plate VII. figs. 121 A. 121 B. 122. 124. bb^2.) and the subsequently vascular lamina of the umbilical vesicle ($bb^{2\prime}$ in the same figures, and in figs. 119. 120. 121; also in Plate VIII. figs. 132 and 150.), appears to me to go very far towards explaining why observers have hitherto supposed the embryo to *arise* in the substance of a *membrane.* I would ask particular attention to the following recapitulation of several of the facts recorded in this memoir; viz. The dark spot designated by Coste the "Tache embryonnaire," obviously corresponds to my "peripheral portion of the germ" (which has the central portion (bb^1) lying under, and often very much concealed by it). The germ sends forth a hollow process (Plate VI. fig. 117. $bb^{2\prime}$.). This process, expanding, receives the yelk into its interior, lines the membrane *am.* as a network (Plate VII. fig. 119. $bb^{2\prime}$. Plate VIII. fig. 132.), and passing through the stages represented in Plate VII. figs. 120 and 121, subsequently assumes the state exhibited in Plate VIII. fig. 150, which appears to be the immediate foundation of the blood-vessels and the blood. The stage shown in Plate VII. fig. 121. is repeated on a smaller scale in fig. 121 A. Here, and in figs. 121 B. and 122, the separate granules (nuclei) surrounded by dark globules ($bb^{2\prime}$) were seen to be a part of the layer constituted by the peripheral portion of the germ (bb^2) (" tache embryonnaire" of Coste). *External* to the structures now described is the membrane *am.* (Plate VI.

[†] Whether circular spaces (Plate VII. figs. 121 A. 121 B. and 122.)—such as those described as incipient in the "Sixteenth Stage" (par. 192.)—in the membrane *am.* existed in the ovum fig. 124, my observations do not enable me to state.

[‡] In these Researches it has not been my practice to make drawings from recollection. Nor have I considered it sufficient to merely sketch the object while it was before me, and subsequently finish it. Precision requires that the drawings should be completed while the object is still in the field of view. On this occasion, however, from the cause above assigned, such a course was impossible; and figs. 124 and 125. were therefore not drawn until after the object had been lost. Fig. 126, as already stated, was taken while the object was still before me.

figs. 113—117. Plate VII. figs. 118. 119. 121 A. 121 B. 122. 124.), i. e. the "serous lamina" of authors, or the subsequently reflected lamina of the umbilical vesicle. *Internal* to those structures, or rather internal to the lamina of the umbilical vesicle, which is subsequently vascular, lies, when formed, the mucous lamina[†].—This, I apprehend, will assist to explain why observers have hitherto supposed the embryo to *arise* in the substance of a *membrane*. It is not a previously existing membrane which originates the germ, but it is the previously existing germ which, by means of a hollow process ($bb^{2\prime}$), originates a structure having the appearance of a membrane[‡].

The Chorion.

221. When in describing the thick transparent membrane of the ovarian ovum in the "First Series" of these Researches (*l. c.* par. 52.), I stated my opinion, in unison with that of Coste and R. Wagner, "that this membrane is really the chorion of ova met with in the uterus," I had not discovered the disappearance of one membrane and the coming into view of another membrane in the Fallopian tube. Such, however, is the fact, as made known in an earlier part of the present paper (pars. 174. 172.); but it is one which did not fall under my notice until near the conclusion of these researches, notwithstanding all the pains that had been taken to procure a consecutive series of stages. It affords evidence that I was formerly mistaken in considering the thick transparent membrane of the ovarian ovum to be identical with the outer membrane of the ovum of the uterus, and "the membrana vitelli [*e*] to be still visible, and to have considerable thickness in minute ova met with in the uterus." It is not that thick transparent membrane itself (*f*) which is identical with the outer membrane, or chorion of the ovum of the uterus, but the thin lamina (Plate VI. fig. 104. α and β. *cho.*) which was seen to come into view on crushing an ovum in a certain state in the Fallopian tube. (The membrane (*e*) of the minute yelk-ball, as already mentioned (par. 174.), disappears by liquefaction during the passage of the ovum through the Fallopian tube.) Those who are practically acquainted with the various difficulties to be surmounted in this branch of physiology, will, I think, be disposed to make allowance for this error[§]. We are now prepared to trace the chorion through its early stages.

222. In Plate VI. fig. 103. α and β. is an ovum found one inch from the infundibulum in the Fallopian tube, at the same time that other ova in a very nearly corresponding state were met with not yet discharged from the ovarium. The next stage is exhibited in fig. 104. α, which presents an ovum taken from the same part of the

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[†] The mucous lamina was possibly incipient in the ovum Plate VII. fig. 118; and if so, it was more advanced in that represented in figs. 120 and 121.

[‡] The "tache embryonnaire," above referred to, appears to have been seen in the Rabbit by several observers. An ovum figured by Dr. Allen Thomson (Edinburgh New Philosophical Journal, vol. 9.) represents it as viewed with a low magnifying power; as does also one figured by my friend R. Wagner (Beiträge, &c., Tab. i. fig. 9.), through whose kindness I had an opportunity of seeing the object itself.

[§] The membrane *f* in the present paper everywhere denotes that which was called the "chorion," and lettered *f* in my "First Series."

tube as the one last mentioned, but in another Rabbit. The membrane *f* was seen to be surrounded by a dark circle, which on the ovum being crushed, as in fig. 104. β, was found to be a thin membrane, *cho.* In fig. 105. are exhibited corresponding parts, with the addition of a fluid (f') between the membranes *cho.* and *f.* This ovum consisted of *three* membranes, *e*, *f*, and *cho.* In fig. 106. the membrane *e* has disappeared, the membranes *f* and *cho.* continuing in nearly the same state as in fig. 105. The suite of observations in later stages is such as to require no explanation, for the two membranes *cho.* and *f* continue very distinctly recognizable. This will be illustrated by reference to the thirteen consecutive stages in figs. 109 to 119. Plates VI. and VII. Those drawings show that no membrane is formed outside the membrane *cho.* during the periods which they represent; and that none such is formed at later periods, up to the time when villi usually make their appearance on the surface of the ovum, I have satisfied myself by careful examination. It may be added, that the same properties characterizing the membrane *cho.* when first seen as a separate structure (par. 172.), have uniformly presented themselves in later stages, viz. great susceptibility of distention, no small degree of elasticity, and a tendency to become elliptical. These are my reasons for believing that the external membrane which becomes villous in the uterus is that which we have seen to be rendered visible as a distinct structure by crushing an ovum (Plate VI. fig. 104. β.) in a certain stage, from the Fallopian tube; a membrane denoted throughout the figures by the letters *cho.*, and designated in the present paper as the chorion[†].

The Chorion becoming Villous.—Mode, Period, and Place of Origin of the Chorion.

223. An early stage in the formation of villi,—the "Saugflocken" of SEILER[‡],—is seen in Plate VIII. fig. 141. The tuft here represented measured in diameter $\frac{1}{50}'''$. It appeared to consist of vesicles containing objects having the form of vesicles. Several of such tufts are shown in profile in fig. 142. Both of these figures were taken from an ovum of 162¾ hours, and measuring $1\frac{1}{2}'''$. I have seen incipient villi on an ovum of $\frac{1}{2}'''$; and I have met with ova of $2\frac{1}{4}'''$ without any. VON BAER observed villi in the Rabbit on an ovum measuring $2'''$; while R. WAGNER on an ovum of the same size, and from the same animal, found none. Thus the period at which villi begin to form, like that of the development of other structures in the ovum (pars. 168. 169.), seems to be subject to considerable variation. The tufts on the above mentioned ovum (figs. 141. and 142.) were situated at unequal distances: some of them nearly

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[†] Does not the structure which in the ova of oviparous animals (the Frog for instance) corresponds to the chorion of Mammalia, arise in the same manner? In many of the Invertebrata this is obviously the case. (Compare with Plate IX. fig. 153. *cho.* in this memoir, several figures in R. WAGNER's Prodromus Historiæ Generationis;—for instance, fig. II^c, V^b, V^c, X^a, XI^a, XIII^c, XVI^b.) In the latter, however, we find the chorion to arise in the ovary. T. WHARTON JONES has pointed out the resemblance in *appearance* between his "gelatinous envelope" in the ovum of the Rabbit and the ovum of the Frog (*l. c.* figs. 1, 2, and 5.).

[‡] Die Gebärmutter und das Ei des Menschen, Dresden 1832, containing excellent representations of the villi of the chorion in later stages.

touching each other, many being about $\frac{1}{36}'''$ apart, and others as much as $\frac{1}{3}'''$ asunder. At the part from which fig. 142. was taken they were most numerous. The villous tufts are yellowish-brown in colour. The very first indication of the formation of villi seems to consist in a few dark globules existing scattered over the surface of the chorion.

224. The mode, the period, and the place of origin of the chorion, are subjects on which physiologists are not agreed. Von Baer† appears to suppose his "sphere creuse à paroi mince" of the ovarian ovum to become the "membrane corticale" (chorion) of ova in the uterus, though he does not express himself with certainty on the subject. Coste‡ and R. Wagner§ consider the thick and transparent membrane (*f*) of the ovum in the ovary to be identical with the membrane called the chorion in the uterus. Purkinje‖, Valentin¶, and Allen Thomson††, maintain that analogy is in favour of the supposition that the chorion originates in the oviduct. Krause‡‡ conjectures that it may be formed after the discharge of the ovum from the ovary, out of the "disc" of granules (my tunica granulosa and retinacula) which surrounds the ovum in that organ. T. Wharton Jones§§ formerly believed the "vitellary membrane"(*f*) to form the chorion, but now supposes "that the gelatinous coat ['proligerous disc'] acquired by the ovum in the ovary, and more especially circumscribed and defined after impregnation, constitutes the only covering of the vesicular blastoderma after the giving way of the vitellary membrane; that this gelatinous-looking coat forms the chorion," &c. My own observations on this subject have been recorded in preceding pages (pars. 172. 173. 221. 222.)‖‖.

225. More particularly, the following are the views of T. Wharton Jones as to the mode, period, and place of origin of the chorion. He says¶¶, "In the ova of the Rabbit, &c. before impregnation, the proligerous disc [= my tunica granulosa and

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† Lettre, &c. Commentaire, pp. 39, 40, 55. Von Baer has since expressed the opinion that in some Mammals, the Hog and Sheep for instance, this membrane arises after the ovum has left the ovary, by the secreted albumen—through a coagulation of its surface—forming for itself an investing membrane. He considers that in the Dog, however, the outer membrane of the ovarian ovum continues the outer membrane of ova in the uterus. (Ueber Entwickelungsgeschichte der Thiere. Beobachtung und Reflexion. Zweiter Theil, pp. 185 to 188, 1837.).

‡ Embryogénie Comparée, p. 80. § Beiträge, &c., p. 36.

‖ Encyclopädisches Wörterbuch, Zehnter Band, p. 128.

¶ Handbuch der Entwickelungsgeschichte des Menschen mit vergleichender Rücksicht der Entwickelung der Säugethiere und Vögel, p. 39.

†† *L. c.*, p. 453. ‡‡ Müller's Archiv, 1837. Heft I. pp. 28, 29. §§ *L. c.*, pp. 339—342.

‖‖ I have in two instances observed the chorion to make its appearance at the surface of the thick transparent membrane *f* ("zona pellucida") in ova still in the ovary and apparently about to be absorbed. Maceration seems sometimes to produce a similar effect. See figs. xviii. and xxii. in Bernhardt's dissertation, "Symbolæ ad ovi mammalium historiam ante prægnationem," Wratislaviæ, 1834; and Edinburgh Medical and Surgical Journal, No. 128, Plate i. fig. 3, 1836, in which the transparent space surrounding the ovum appears to me to represent the fluid (*f'* in my figures) imbibed by the chorion, the latter being perhaps hidden by the surrounding granules of the "zona granulosa" (my tunica granulosa).

¶¶ *L. c.*, p. 340.

retinacula, Plate V. fig. 96. g^1. and g^2.] in which the ovum is imbedded is observed to be composed of a gelatinous substance interspersed with grains, but as yet there appears no distinctly circumscribed envelope." T. WHARTON JONES then refers to the views of KRAUSE, and remarks†, "From his [KRAUSE'S] observations on the ovum *before* impregnation, he has been led to form much the same opinion regarding the origin of the chorion as is recorded in this memoir." This opinion of KRAUSE appears to have been the following‡. "It may be conjectured that the ovulum on the bursting of the folliculus passes with the disc and layer of albumen into the Fallopian tube, and that *out of the granules of the former* [i. e. the "disc"] the chorion is formed." My own observations do not realise the conjectures of T. WHARTON JONES. On the contrary, they show that when the chorion first comes into view, it is not as a "gelatinous coat§," but in the form of a thin lamina *closely investing* the thick transparent membrane or "zona pellucida" (Plate VI. fig. 104. α and β. *cho.*); and that this thin lamina—*itself the incipient chorion*—expanding from the "zona pellucida," imbibes a quantity of fluid into its interior, thickens, and with the imbibed fluid presents a gelatinous appearance,—but that the chorion is not formed out of the gelatinous-looking "coat§", since the outer portion of this "coat" is *from the first* constituted by a membranous structure, the chorion,—and the imbibed fluid which formed the principal part of the "coat" (Plate VI. fig. 105 to 113. *f.*) soon passes into the interior of the ovum, leaving the chorion again in close contact with the outer surface of the "zona pellucida" (Plate VI. fig. 117. *cho.* and *f.*) The conjecture of KRAUSE, however, does not appear to me to coincide with that of JONES, so closely as the latter seems to have supposed. So far from this, I think it by no means improbable that—as conjectured by KRAUSE—the so-called "disc" (my tunica granulosa and retinacula) may bring from the ovary the materials out of which the chorion is formed, and it is possible that the granules (vesicles) of the "disc" may coalesce to form it. Thus that portion of the tunica granulosa (g^1) which in the ovum Plate IX. fig. 153. was seen surrounding the incipient chorion (*cho.*) on its rising from the membrane *f*, may have been destined to enter into the formation of the chorion, and to contribute towards the thickening of this membrane, as well as to supply fluid for its imbibition (par. 150. 151.)‖.

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† *L. c.*, p. 340.

‡ MÜLLER'S Archiv, 1837, Heft I. pp. 28, 29.

§ See his fig. 1. *l. c.* plate xvi. See also figs. 109. and 110. in Plate VI. of the present memoir.

‖ I am inclined to think that the "very delicate albuminous membrane" figured by KRAUSE (*l. c.*, Taf. I. figs. 4 to 6.) as surrounding a "thin layer of fluid albumen," must have been the incipient chorion (from some cause making its appearance in the ovary) though KRAUSE does not seem to have regarded it as such (compare with Plate IX. fig. 153. in the present memoir). On a former occasion (*l. c.*, par. 49. *Note.*) I stated that an examination of the ovum of the Goat enabled me to attest the accuracy of KRAUSE; but to a certain extent only; for I did not "in any instance find the membrana vitelli surrounded by a fluid as described by KRAUSE, but by the perfectly formed and consistent chorion" (as I then called the membrane *f.*). From this, however, it will be obvious that the "exceedingly distinct" membrane, which I found to circumscribe the yelk in ova of this animal, cannot have corresponded, as I then believed, to the thick membrane figured by Professor KRAUSE in the same situation.

225½. The names that have been given to the outer membrane of the unattached ovum in the uterus, are very numerous. That membrane appears to be the "Exochorion" of **Burdach** and of **Velpeau**,—the "Membrana corticalis," "Schalenhaut," and "future Exochorion" of **Baer**,—the "Chorion," "Eihaut" and "future Exochorion" of **Valentin**,—the "Membrane vitelline" of **Coste**,—and the "Chorion" of **Purkinje**, **R. Wagner**, **Allen Thomson**, and **T. Wharton Jones**.

Aborted Ova.

226. Ova apparently aborted are shown in Plate VIII. figs. 133 to 135. These were found in the uterus of different individuals. At fig. 133. is one of *two* ova that were aborted, the development of others found in the same uterus having proceeded duly. I have met with three instances in which *all* the ova found in the same individual appeared to be aborted. See figs. 134 and 135. In the latter figure a vesicle, apparently corresponding to that which I have described as containing the germ (par. 186.), was seen to occupy the centre of the ovum, having a few scattered vesicles around it. This was the case in all the ova (five) found in the same uterus. In some instances of aborted ova (figs. 133 and 134.) the chorion had not made its appearance as a separate structure. In other instances (fig. 135. *cho.*) it had come into view. The aborted ova which I have met with, whether exhibiting a chorion or not, were considerably smaller than is usual in regularly developed ova of corresponding periods. In fig. 133, the yelk-ball was in the state in which I have met with it in the Fallopian tube, in early stages, except that it was much smaller and elliptical. In one instance, fig. 134, two objects resembling the ovarian retinacula were present, but the tunica granulosa had disappeared. When ova were found aborted, I generally examined the ovary, but nothing abnormal was in any of those instances observed in the ova still present in that organ. It is remarkable, that of the ova met with in these researches in the uterus, no fewer than one in about eight should have appeared to be aborted.

227. On one occasion I found six or seven vesicles in the Fallopian tube of $\frac{1}{8}'''$ (Plate VIII. fig. 136.), having a thick transparent membrane, and containing a colourless and pellucid fluid. In some instances granules were observed on their inner surface†.

Effects produced on Ova by Manipulation.

230. Ova of the Rabbit belonging to the periods chiefly considered in this paper are globular, but very little pressure renders them elliptical. The tendency to this change in form is most obvious in ova that have reached the uterus, and appears indeed to be in proportion to their size. This tendency appears to me to lie chiefly in the chorion (Plate VIII. fig. 128. *cho.*), which may be found deserving of notice. **Prevost**

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† On another occasion upwards of thirty vesicles were observed in the Fallopian tube, measuring from $\frac{1}{12}'''$ and less, to $\frac{1}{70}'''$. These contained also a pellucid fluid, and the larger ones had a thick membrane like that in Plate VIII. fig. 136. The membrane of the minuter ones was very thin.

and **Dumas**, as well as **Coste**, observed a tendency in the mammiferous ovum to become elliptical. The former also state that ova from the horns of the uterus in Dogs are at first (that is in an unaltered state) elliptical†.

Effects produced on Ova by certain Chemical Reagents.

231. The mammiferous ovum in its most interesting state being, from its minuteness, very difficult to obtain and also very perishable, it has appeared to me important to discover some medium in which, when found, its examination might be more perfectly accomplished. The smallest ova from the Fallopian tube and uterus, it is my practice to view imbedded in some of the mucus taken from those parts. The larger ova require a transparent fluid to support them. Water does not answer well. Its operation on ova of $1'''$ to $1\frac{1}{3}'''$ appears to be as follows (Plate VIII. fig. 137.): first, the inner membranes (amnion and umbilical vesicle) separate for the most part from the membrane *f*, leaving the latter on the inner surface of the chorion. This separation is produced, not by imbibition, (for the chorion does not at first enlarge,) but by the passage outwards of a portion of the fluid yelk, which now lies between the amnion and the membrane *f*. Secondly, folds appear in the chorion, their direction coinciding with the longitudinal diameter of the now elliptic (par. 230.) ovum, which soon collapses in a shell-like form (fig. 137.).

232. I have tried solutions of various chemical reagents. Some of these occasioned collapse of the chorion. Others coloured it highly, which was found a disadvantage, although that colour was transparent. Another set rendered it opake. By some it became constricted. Others caused too entire a separation of the internal membranes from the chorion. Now all of these are unfit for the purpose. We require a reagent that does not materially affect the chorion, and yet one that renders more distinct the internal objects sought for.

233. The effect of spirit of wine was found to vary with its strength. Rectified spirit (sp. gr. 0·835) acted too energetically, rendering the chorion in some degree opake. Dilute spirit facilitated for a while the examination of the interior of the ovum, by increasing its transparency‡, and the chorion remained unchanged. Considerable collapse, however, of the inner membranes followed. The dilute spirit here mentioned had a sp. gr. of 0·950, and it was of this strength when used combined with other substances.

234. Brine of common salt produced immediate and entire collapse of the internal membranes in ova of $2\frac{1}{2}'''$; the chorion continuing for a while unchanged.

235. I might go on to enumerate the effects of various other reagents, such as ether and hydrocyanic acid; the nitric and acetic acids; sulphurous acid and sulphuretted hydrogen (deoxydizing substances); solutions of various metals, among which were

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† *L. c.* No. 189, p. 200.

‡ The internal membranes of minute ova which had been collapsed by water, I have observed to recover themselves on the addition of dilute spirit; but this effect appeared to be of short duration.

mercury, silver, lead, and gold; the acetate and potash-sulphate of alumine; solutions of nutgalls, and of pure tannin, as being astringent; solutions of sulphated indigo and of logwood; and lastly, aqueous solutions of chlorine and of iodine. Some of these produced interesting though unimportant results, but none of them fulfilled all the conditions above indicated.

236. The action of corrosive sublimate (*hydrargyri bi-chloridum*), as the usual test of the presence of albumen, deserves particular notice, because that after an ovum had been digested twenty-four hours in a weak solution of that salt, its transparency was unimpaired, although the internal membranes were contracted and corrugated. That ovum measured $\frac{1}{3}'''$. Another measuring $\frac{1}{4}'''$ was subjected to the action of the same salt with a similar result. Both of these ova were taken from the same individual.

237. Among the most remarkable effects, are those produced by lead and silver. The action of lead, unlike that of corrosive sublimate, seems to be upon the chorion itself, rendering it opake and white, and therefore intercepting the view of the interior. This effect was almost instantaneous. It was the acetate of lead that was employed, to which enough dilute spirit had been added to make the mixture not more than slightly sweetish.

238. Nitrate of silver, diluted with spirit as above, immediately produced the appearance of a beautiful network in the interior of the ovum (Plate VIII. fig. 140.). This was the effect of its action on the membrane of the vesicles constituting the outer lamina of the future umbilical vesicle. Very shortly, the interior of those vesicles became remarkably distinct. See Plate VIII. fig. 150, taken from an ovum of $\frac{1}{5}'''$ as seen lying in a solution of this salt. Nitrate of silver when used for the examination of the vesicles in question while still within the ovum, should be sufficiently diluted, or it will render the chorion opake. If weak, its action seems to be more perceptible upon the interior of the ovum than upon the chorion. Three ova after remaining in the above solution of silver twenty-four hours, had become of a purple brown colour; solar light having been admitted into the room.

239. I have lately chanced to see Professor Johann Müller's paper† recommending kreosote water as a medium for preserving nervous substance, and have been induced to try its effect upon the ovum. It answers well, as will be obvious on reference to Plate VIII. fig. 138.; this drawing having been taken after the ovum had lain three days in that fluid. I recommend a saturated aqueous solution of kreosote in preference to any other medium, for the examination of the entire ovum. Its operation on ova of about $\frac{1}{10}'''$ is as follows. The inner membranes (see the figure) recede to some extent from the membrane *f*, which continues in contact with the chorion (*cho.*). The chorion generally remains unaltered in both form and size. The change, therefore, consists, not in imbibition from the exterior‡, but in the passage outwards of a portion

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† Müller's Archiv, 1834, Heft I.

‡ In a very minute ovum, however, immersed in kreosote water, I observed some imbibition from the exterior and consequent enlargement.

of the fluid yelk (*d*) which now lies between the amnion (*am.*) and the membrane *f*. Besides a separation from the membrane *f*, the receded membranes exhibit some separation from one another. See Plate VII. fig. 121 A. representing an ovum in which, by kreosote water, a change of this kind was produced, just sufficient to make the true nature of the interior of the ovum obvious, including the vesicles of the embryo (bb^1 and bb^2) and the nuclei composing the outer lamina of the umbilical vesicle ($bb^{2'}$), to which it imparted a slight tinge of yellow. Instead of using (as mentioned above, par. 231.) the mucus of the uterus for the examination of the minuter ova found in that organ, I have sometimes employed kreosote water in the following manner. A minute portion of this fluid having been placed upon a plane glass surface, the ovum, first freed by bibulous paper from the adherent mucus, is introduced into it, and thus examined either with or without the compressor. If the compressor be not employed, it is important by means of a very fine hair pencil to add kreosote water frequently, so as to keep the ovum from becoming dry.

240. Tar water has advantages in producing no collapse of any part. See Plate VIII. fig. 139., drawn after an ovum had lain four days in this fluid. It appears, however, to constrict the chorion, and it has the effect of colouring too highly.

241. For preserving ova I have tried several fluids. Dilute spirit and kreosote water seem each to answer pretty well; but there occurs eventually too great collapse of the inner membranes. (See par. 313. and third Note.)

Some of the earliest appearances of the Ovum.

242. In Plate V. figs. 82 to 84, are objects apparently representing stages in the formation of the mammiferous ovum even earlier than any of those met with in the "First Series" of these researches. Of the objects now referred to, which were met with incidentally, those in nearly the same stage were observed to lie together in a group. The three groups of which specimens are given in the above figures were in the immediate neighbourhood of one another, and they were all contained in a fluid substance. The most primitive of these three conditions appears to be that represented in fig. 82., in which are vesicles surrounded merely by dark granules or globules. The largest vesicle in this figure had a diameter of only $\frac{1}{100}'''$. I have seen vesicles having a similar appearance, in the same group, measuring no more than about $\frac{1}{300}'''$; and from their external appearance I am ready to suppose that they were compound, consisting of at least two membranes, the one closely invested by the other. The next stage noticed, is that in fig. 83. Here were found central vesicles having the same appearance of a compound structure as the foregoing, but being in general larger (the largest in the figure measured $\frac{1}{75}'''$), and each presenting an envelope of smaller vesicles. Among the latter were many dark granules or globules. In fig. 84. is seen a stage somewhat more advanced. The central vesicles had essentially the same appearance as the foregoing, but were generally larger (the largest in the figure was about $\frac{1}{30}'''$), and their envelopes of vesicles seemed more perfectly formed, and were

free from the dark granules or globules of the preceding stage. One of the central vesicles in this figure presented an indistinct spot. The object in fig. 81. is from the ovary of the common fowl. Its central part resembled the objects in figs. 83 and 84. from the Rabbit (par. 292. *Note.*).

Facts noticed by preceding Observers, cited to show the Accordance in various respects between the Development of Mammalia and that of other Animals.

243. I have copied from Carus[†] the drawing of a structure (Plate VIII. fig. 131.) occurring in the ovum of one of the Mollusca,—*Unio tumida.* With this I would compare fig. 130. The latter represents a structure of the same kind as that which in Mammalia closely invests the embryo, and seems to enter into the formation of the *amnion*; the former exhibits an object which, in the molluscous animal above-mentioned, appeared to Carus to be the foundation of the *shell.* The Professor saw a similar structure in ova of *Anodonta.* The "rhombick fields" in the figure from Carus, as that author terms them, (which appear to me to have been vesicles with nuclei) seemed to *become more numerous* as development proceeded. With this observation I would compare what has been stated (par. 174 to 178. 180. 314 to 318.) regarding the greater number and smaller size of the successive sets of vesicles in the mulberry-like structure met with in several stages of the mammiferous ovum, (Plate VI. figs. 105½ to 110., Plate VIII. fig. 130.).

244. In the ovum of some of the Crustacea, there occurs a striking resemblance in appearance to the structure, just referred to in the Mammalia, when it has attained a more advanced stage. Such for instance is the case in the ovum of the Crafish, *Astacus fluviatilis*[‡]. Spiders present in their ova a resemblance equally remarkable. With Plate VI. fig. 114. *am.* I would compare, for example, the ova of *Epeira diadema,* as figured by Herold[§]. Now in those animals, the structure which I suppose to correspond to that marked *am.* in my figures, appears to enter into the formation of the *outer covering of the abdomen.*

245. In none of the figures of the authors last referred to, have I been able to discover any trace of what seems to me entitled to be denominated a "germinal membrane." But, on the other hand, there is to be recognised in a great number of those figures, an unquestionable similarity at the part where the embryo arises, to the corresponding part in the ovum of Mammalia.

246. Rathke describes a depression in the ovum of the Crafish[‖], and also in that of *Bopyrus squillarum*[¶], which in some respects appears closely to resemble that which I suppose to be the corresponding part in Mammalia, as shown in Plate VI. fig. 114. *a. p.*

[†] Neue Untersuchungen über die Entwickelungsgeschichte unserer Flussmuschel, Tab. II. fig. 1.

[‡] Rathke, Flusskrebs, Tab. I.

[§] Untersuchungen über die Bildungsgeschichte der Wirbellosen Thiere im Eie, Theil I.

[‖] Flusskrebs, Tab. I. fig. 2.

[¶] Zur Morphologie Reisebemerkungen aus Taurien, p. 44.

Of *Bopyrus* he says, "when the formation of the embryo is about to commence, there arises on one part of the yelk a minute, pellucid, and perfectly colourless spot"†. "Where this spot is, there is presented a long and broad, but very shallow depression."—"The above-mentioned colourless spot does not indicate a very great thickening of the germinal membrane, but only a space behind the external membrane of the ovum (Eihaut), which is perhaps filled with a fluid as clear as water, for after the operation of diluted nitric acid that part had not in the least lost its transparency." The same author describes in the Amphipoda‡ a "tray-like depression" as occurring on the surface of the ovum. In certain Decapoda§ (*Crangon* and *Palæmon*) he mentions "a place where the yelk to some extent is somewhat flattened, having at the same time receded, and where a disc of little thickness, consisting of a substance as transparent as the purest glass, lies upon the yelk."

247. In ova of the Actinææ, **Rathke** found that *no "germinal disc" or "germinal membrane" arose* which could develope itself into an embryo; and hence concludes that here the formation of the new being must take place in a manner entirely different from that which occurs in Mollusca, Insecta, Crustacea, and Vertebrata‖.

249. The acknowledged existence of an "area pellucida,"—for instance in the ovum of the Bird,—is opposed to the present views of authors, that the embryo arises in the substance of a membrane; but it accords perfectly with the fact (Plate VI. fig. 114.), that the germ or future embryo of Mammalia is contained in such a pellucid area.

250. **Rathke** in a recent paper¶, though not doubting that the "primitive trace" is constituted by the "central portion of the germinal membrane" (for he uses these terms synonymously), makes known the fact,—observed by him in Mammals, Birds, and some Reptiles,—that parts previously supposed to be formed by what **Baer** had denominated the "laminæ ventrales of the serous lamina of the germinal membrane," really originate independently of any membrane. These parts are the ribs and pelvic bones and the muscles of the thorax and abdomen, which, according to **Rathke**, arise in a newly added substance "proceeding out of the primitive trace," and pushing the membranous "laminæ ventrales" farther and farther from the latter. Observations previously made by **Reichert**†† are referred to by **Rathke** as being in accordance with the above. **Reichert** had found that the part originating the lower jaw and hyoid bone "grows out of the primitive trace." Now if, from the facts made known in the present memoir, it should appear that the "primitive trace" is not constituted by the central portion of a membrane, it will be easy to understand that the parts above referred to by **Reichert** and **Rathke** should have an origin equally independent of a membrane.

251. An appearance in the development of the Frog and Salamander has been thus

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† Zur Morphologie, &c., Tab. II. fig. 1.

‡ *Ib.*, p. 74.

§ *Ib.*, p. 83.

‖ Zur Morphologie, &c. p. 15.

¶ Müller's Archiv, 1838, Heft IV.

†† Ueber die Visceralbogen der Wirbelthiere, Berlin, 1837.

described by Rathke[†]. "Soon after the development of the ovum of these amphibia has commenced, the germinal membrane *becomes in one part more and more thickened*, and the thickening presents itself under the form of a moderately broad lamina, extending about the distance of from one pole of the yelk to the other[‡]. *Around this lamina, which is the primitive portion of the embryo*, the germinal membrane becomes somewhat drawn in; and at this time the two ends of the lamina (which are the cephalic and caudal extremities of the embryo) *grow forth like two liberated processes* out of the germinal membrane, becoming at the same time curved so as to approach one another around the yelk, as it were striving more and more to embrace the latter." The "thickening" here mentioned, of the supposed "germinal membrane," and the consequent appearance of a "*lamina*," the two ends of which "*grow forth like liberated processes*," admit, I respectfully suggest, of a different explanation. That "*lamina*," the "*primitive portion of the embryo*," appears to me to correspond in its situation and manner of growth to the *germ*, which it has been my endeavour to show is the foundation of the mammiferous embryo. The membrane, however, (*am.* in Plates VI. and VII.) in Mammalia which becomes "drawn in" merely *invests* the embryo, and is *not continuous with it* (see par. 220.). Is it not really so with the "lamina" in question, in the amphibia above-mentioned, as well indeed as with the corresponding part in other animals?

252. Judging from my observations made on the mammiferous ovum, I am convinced that Baumgärtner[§] has very accurately described a comparatively early appearance of the embryo of the Frog, as regards both form and substance.

253. It has been conceived[‖] that in vertebrated animals the brain and spinal cord form on the *outer* side of the so-called "serous lamina of the germinal membrane," the foundation of the extremities being situated on the *tube* which that "membrane" has been supposed to form; but that in Invertebrata, the chain of ganglia arises on the *inner* or yelk side of that "lamina," and that the extremities on the other hand make their appearance on its *outer* side. I would submit, however, that the difficulty here has been in no small degree attributable to the prevailing opinion that the embryo arises in the substance and by the foldings of a membrane.

Recapitulation.

254. The difference perceptible between the mature and immature ovum consists in the condition of the yelk; the yelk of the mature ovum containing no oil-like globules (par. 120. 122. and *Note*. 124. *Note*.).

255. Both maceration and incipient absorption produce changes in the unimpreg-

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[†] Flusskrebs, p. 86.

[‡] The Professor here refers to the authority of Prevost and Dumas, of Rusconi, and of Baer.

[§] Müller's Archiv, 1835, Heft VI. The previous work of Baumgärtner, on the Frog, I regret to say I have not seen.

[‖] Rathke, Flusskrebs, pp. 77 to 91.

nated ovum, which in some respects resemble those referable to impregnation (par. 123. 124. 160.).

256. During the Rut, the number of Graafian vesicles appearing to become prepared for discharging their ova, exceeds the number of those which actually discharge them (par. 125.).

257. Ova of the Rabbit which are destined to be developed, are in most instances discharged from the ovary in the course of nine or ten hours *post coitum*; and they are all discharged about the same time (par. 128. 130.).

258. There is no condition of the ovum, uniform in all respects, which can be pointed out as the particular state in which it is discharged from the ovary; but its condition in several respects is very different from that of the mature ovum *ante coitum* (par. 140.).

259. Among the changes occurring in the ovum before it leaves the ovary, are the following, viz. the germinal spot, previously on the inner surface, passes to the centre of the germinal vesicle; the germinal vesicle, previously at the surface, passes to the centre of the yelk; and the membrane investing the yelk, previously extremely thin, suddenly thickens. Such changes render it highly probable that the ovary is the usual seat of impregnation. This opinion is not incompatible with the doctrine that contact between the seminal fluid and the ovum is essential to impregnation, since in the course of these researches it has been found that spermatozoa penetrate as far as to the surface of the ovary (par. 143 to 147.)†.

260. The retinacula and tunica granulosa are the parts acted upon by the *vis a tergo* which expels the ovum from the ovary. These parts are discharged with the ovum, render its escape gradual, perhaps facilitate its passage into the Fallopian tube, appear to be the bearers of fluid for the immediate imbibition of the ovum, and probably enter into the formation of the chorion (par. 148 to 151. 225.).

261. After the discharge of the ovum from the ovary, the ovisac is obtainable free from the vascular covering, which together with the ovisac had constituted the Graafian vesicle. It is the vascular covering of the ovisac which forms the *corpus luteum*. Many ova, both mature and immature, disappear at this time by absorption (par. 154. 156. 158.).

262. In some animals, minute ovisacs are found in the infundibulum; the discharge of which from the ovary appears referable to the rupture of large Graafian vesicles, in the parietes or neighbourhood of which, those ovisacs had been situated (par. 162 to 165.).

263. The diameter of the Rabbit's ovum when it leaves the ovary, does not generally exceed $\frac{1}{12}$ of a Paris line (= about $\frac{1}{135}$ of an English inch), and in some instances it is still smaller (par. 166.).

264. The ovum enters the uterus in a state very different from that in which it leaves the ovary. Hence the opinion of **Baer**, that "in their passage through the

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† See the Notes to par. 145 and 278.

tube the ova of Mammalia undergo scarcely any metamorphosis at all," is erroneous (par. 181 to 183.). Among the changes usually taking place in the ovum during its passage through the Fallopian tube are the following; viz. 1. an outer membrane, the chorion, becomes visible; 2. the membrane originally investing the yelk, which had suddenly thickened, disappears by liquefaction; so that the yelk is now immediately surrounded by the thick, transparent membrane ("zona pellucida") of the ovarian ovum; 3. In the centre of the yelk, there arise several† very large and exceedingly transparent vesicles. These disappear and are succeeded by a smaller and more numerous set. Several sets thus successively come into view, the vesicles of each succeeding set being more numerous and smaller than the last, until a mulberry-like structure has been produced, which occupies the centre of the ovum. Each of the vesicles of which the surface of the mulberry-like structure is composed, contains a colourless and pellucid nucleus; and each nucleus presents a nucleolus (par. 170 to 180.).

265. In the uterus, a layer of vesicles of the same kind as those of the last and smallest set here mentioned, makes its appearance on the whole of the inner surface of the membrane which now invests the yelk. The mulberry-like structure then passes from the centre of the yelk to a certain part of that layer (the vesicles of the latter coalescing with those of the former, where the two sets are in contact, to form a membrane—the future amnion), and the interior of the mulberry-like structure is now seen to be occupied by a large vesicle containing a fluid and dark granules. In the centre of the fluid of this vesicle is a spherical body, composed of a substance having a finely granulous appearance, and containing a cavity filled with a colourless and pellucid fluid. This hollow spherical body seems to be the true germ‡. The vesicle containing it disappears, and in its place is seen an elliptical depression filled with a pellucid fluid. In the centre of this depression is the germ, still presenting the appearance of a hollow sphere (par. 184 to 186. 190. 189.)§.

266. The germ separates into a central and a peripheral portion§, both of which at first appearing granulous, are subsequently found to consist of vesicles. The central portion of the germ occupies the situation of the future brain, and soon presents a pointed process. This process becomes a hollow tube, exhibiting an enlargement at its caudal extremity, which indicates the situation of the future sinus rhomboidalis. Up to a certain period new layers come into view in the interior of the central portion of the germ, parts previously seen being pushed further out (pars. 193. 209. 197. 214. 212.).

267. From the region occupied by the germ there issues a hollow process, which by enlargement is made to line the inner surface of the ovum,—that is to say, the

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† There arise at first two vesicles, then four or more, and so on. (See par. 174 and second Note, also par. 314 to 318.)

‡ See, however, the first part of the Note to par. 186.

§ See, however, the Notes to par. 186. 193. 197.

inner surface of the membrane entering into the formation of the amnion (which corresponds to the "serous lamina" of authors), and the process now lining it represents an incipient state of the subsequently "vascular lamina" of the umbilical vesicle —a lamina continuous with the structure corresponding to the "area vasculosa" of authors on the Bird (pars. 193. 201 to 204. 211. 216. 219. 220.).

268. There does not occur in the mammiferous ovum any such phenomenon as the splitting of a "germinal membrane" into the so-called "serous, vascular, and mucous laminæ" (par. 194 to 196.).

269. Nor is there any structure entitled to be denominated the "germinal membrane;" for it is not a previously existing membrane which originates the germ,—but it is the previously existing germ which, by means of a hollow process (par. 267.), originates a structure having the appearance of a membrane (par. 220.).

270. The structure entering into the formation of the amnion is no part of that which constitutes the embryo (pars. 199. 200.). The first appearance of the amnion is in the form of an epithelium (par. 184.). From the delineations of authors it appears to be a corresponding structure, which in Mollusca is the foundation of the shell, and in Crustacea and Arachnida that of the outer covering of the abdomen (pars. 243. 244.).

271. The most important of the foregoing facts respecting the development of the mammiferous ovum, however opposed they may be to received opinions, are in accordance with, and may even explain, many observations which have been made on the development of other animals, as recorded in the delineations of preceding observers (par. 243 to 253.).

272. The ovum may pass through at least one-and-twenty stages of development,—and contain, besides the embryo, four membranes, one of which has two laminæ,—before it has itself attained the diameter of half a line (par. 207.), a fifth membrane having disappeared by liquefaction within the ovum (par. 174.).

273. The size of the minute ovum in the Fallopian tube and uterus affords no criterion of the degree of its development; nor do any two parts of the minute ovum in their development necessarily keep pace with one another. The embryo sometimes attains a considerable degree of development in its form when exceedingly minute (pars. 218. 219. 167. 168.).

274. The proportion of ova met with in these researches in the uterus which seemed to be aborted, has amounted to about one in eight (par. 226.).

275. With slight pressure the ovum, originally globular, becomes elliptical. Its tendency to assume the latter form is referable chiefly to a property of the chorion, and seems to be in proportion to the size of this membrane (par. 230.).

276. The chorion is not—as was supposed in the "First Series" of these Researches —the thick transparent membrane ("zona pellucida") itself of the ovarian ovum, but a thin lamina which rises from the surface of that membrane, thickens, imbibes fluid, and is for a while separated from that membrane by the imbibed fluid. The fluid

then passes into the interior of the ovum, leaving the chorion again in close contact with the membrane from which it rose. That the lamina in question is really the chorion, has been shown by tracing it from its origin in the Fallopian tube up to the period when it becomes villous in the uterus (pars. 172. 173. 178. 180. 182. 221 to 225½.).

POSTSCRIPT.

277. It was not until after the foregoing memoir had been presented to the Royal Society, that a recent work by Professor RUDOLPH WAGNER, on the Physiology of Generation and Development[†], came into my hands. That volume contains an account of some researches by BISCHOFF on the ovum of the Dog in the Fallopian tube and uterus, which (though the plates intended to illustrate them have not yet appeared) it may be proper to notice here.

278. It is easy to understand that the ova of the Dog and Rabbit should, even at a very early period, be distinguishable by peculiarities in their development; but it is difficult to account for differences so remarkable as those which present themselves on comparing the statements of BISCHOFF with my own. There are only two observations, indeed, in which we seem to be agreed; viz. that the Spermatozoa penetrate as far as to the ovary[‡], and that ova are often found lying near together in the Fallopian tube. In other respects there appears to be little or no agreement between the observations we have respectively recorded. Several of these, however, relate to points of cardinal importance. Thus BISCHOFF maintains that the ovum receives no new external covering, but that the "zona pellucida" of the ovarian ovum is identical with the chorion of the uterus. This, as already stated, was formerly my own opinion; and so nearly are the disappearance of one membrane and the coming into view of another membrane simultaneous, that it was not until near the close of these Researches that I was undeceived (par. 221.). BISCHOFF states that during the passage of the ovum of the Dog through the tube, the yelk undergoes changes in its form, becoming angular; and he mentions certain granulous rings of what he supposes the second or inner membrane; also that in some ova *from the uterus* he found the yelk uniform (in its consistence) and opake. I refer to my own figures for an explanation of those apparent rings; and am compelled to add, that in the ovum of the Rabbit I have not met with any solid substance uniform in its consistence, for a considerable period *before the exit of the ovum from the Fallopian tube.* Can it have been that the

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[†] Lehrbuch der Physiologie, Erste Abtheilung, Physiologie der Zeugung und Entwickelung. Leipzig, 1839. This work contains within the small compass of 140 pages, a luminous account of the present doctrines on generation and development, together with a number of new facts observed by the author.

[‡] A Note has been added in a preceding page (par. 145.), acknowledging that BISCHOFF preceded me in this discovery. Towards the end of June 1838 he observed Spermatozoa on an ovary of the *Dog*. On the 5th of the following September I found those animalcules on an ovary of the *Rabbit*, and the next day (Sept. 6) observed a single Spermatozoon on the ovary of another Rabbit. Early in the following December R. WAGNER found Spermatozoa in the Dog between the fimbriæ, close to the ovary.

successive groups of vesicles (Plate VI. figs. 105½ to 110.) which occupy the centre of the ovum (and are finally (fig. 113.) found to contain the germ in their interior) were described by BISCHOFF as an "angular" yelk? That observer recognises a "germinal membrane" in the ovum of the Dog. According to my observations the embryo of the Rabbit does not arise in the substance of a membrane†.

279. In addition to the foregoing statements of BISCHOFF, it may not be improper to refer to existing views rather more fully than I have hitherto done; from which it will perhaps appear that the difference between many of those views and the opinions I have been compelled to form, is mainly attributable to the absence of a suite of early stages.

280. R. WAGNER, in the latest work that has appeared on the subject‡, remarks as follows. "In mature ova, impregnated or susceptible of impregnation, the germinal vesicle disappears. How this takes place, whether in consequence of a sudden bursting, or through rapid dissolution and liquefaction, flattening down, diminution of its contents, &c., does not admit of being with certainty determined; so much is certain, that the germinal vesicle has always disappeared as soon as the ovum has left the ovary. On the immediate changes consequent upon the disappearance of the germinal vesicle we have no more than mere hypotheses, for observation has hitherto given no explanation of them." The same eminent author subsequently remarks, "Of all occurrences in the history of development, the reception of the ovum into the tubes, and its progress until it reaches the uterus, is the most veiled in obscurity. In Man no one has yet succeeded in observing ovula in the tubes: and it belongs to the most difficult and delicate of anatomical examinations to discover objects so minute even in Mammals, in which the time of impregnation may be so exactly known. In Rabbits the ovula pass into the uterus from three to five days after impregnation, in Dogs from ten to twelve days. In these animals—whose earliest period of development is the best known—several ova are separated and pass in succession—rarely together—into the uterus; and it would seem that the Graafian vesicles also often do not all burst at the same time, but that single ones may burst several days before the rest. The ovula undergo but little change during their passage through the tubes; they take with them from the Graafian vesicle a part of the granulous stratum, which as an irregular, lacerated, disc-like structure, remains adherent to them, but is soon stripped off§." "As soon as the ovula have passed out of the oviduct into the uterus, they undergo the first remarkable changes, and grow rapidly. The following applies chiefly to the Dog and Rabbit, whose ova have been the most frequently examined.

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† Since the above was written I have seen the plates of R. WAGNER's work, referred to in par. 277 (Icones Physiologicæ. Fasciculus primus, 1839). They are truly admirable. In reference, however, to the Figures from Professor BISCHOFF (tab. VI. fig. III. to VIII.), I am compelled to state my belief, that while they serve to show this very talented observer to be in advance of others in his acquaintance with the mammiferous ovum, they also confirm an opinion I had previously formed—that he did not obtain a suite of early stages.

‡ Lehrbuch der Physiologie, 1839, pp. 57, 58.

§ p. 94.

The ova, which in the ovary measured only $\frac{1}{10}$ to $\frac{1}{12}$ of a line, have reached the size of $\frac{1}{2}$ to $\frac{3}{4}$ of a line. It is distinctly seen that the chorion distends and becomes thinner; the yelk-ball swells, but becomes more fluid; as the dark granules disappear larger drops of oil come into view; at the same time the superficial granulous layer of the yelk acquires a membrane-like consistence, and the granules present little accumulations, which are insulated, and soon form, at a certain place, an opake circular spot, which in ova measuring a line is perceptible as a minute point with the naked eye. This spot consists of a stronger aggregation of granules, which rises somewhat scutiform—being thicker than the remaining membrane—and soon becomes rather more pellucid in the centre, while the granules group themselves wall- or ring-like at the circumference. The granules of this disc appear to be distinct cells [vesicles], having in the centre a minute opake nucleus. The double membrane of these minute ova is seen very distinctly when the ova have been but a few seconds in water; the outer perfectly transparent membrane—which has no distinct structure—separates very quickly from the inner, which invests the yelk and bears the granulous spot. The outer membrane is the former chorion ['zona pellucida'], the inner is the germinal membrane, blastoderma, which as a membrane has already grown around the entire yelk, and perhaps at an earlier period—as a continuous granulous layer—invested the same, while in the Bird as a disc-like object it occupies only a minute space on the yelk. Probably the space between the two membranes is filled with a thin layer of albumen, which the ovulum has acquired in the oviduct and uterus, and which swells by imbibition in water, producing a greater separation of the two membranes. The granulous spot is the place from which the formation of the embryo proceeds, and hence has by some been called the embryonal spot (tache embryonnaire of Coste). Whether the outer membrane continues to be the most external and becomes the villous chorion, or whether yet a delicate membrane, as exochorion, forms around it, which some maintain, is doubtful; but the former is the more probable. The ova still lie loose in the uterus, and have passed from the round to the oval form†."

Rotatory Motions of a Mulberry-like Object in Vesicles under the Mucous Membrane of the Uterus.

281. In the course of my researches on the mammiferous ovum I have seen in the uterus of the Rabbit minute pellucid vesicles under the mucous membrane of that organ. These vesicles are frequent in that part of the uterus (under its mucous membrane) which joins the Fallopian tube. By enlargement they resemble ova; and the observer is sometimes not undeceived until he attempts to lift them from their situation. The uterus has generally been more or less highly vascular when the subject of my examination. Whether the vesicles in question present themselves in other states of this organ I do not know; but on the inner surface of what seemed one of

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† pp. 97, 98.

those vesicles was seen a beautiful display of newly-formed vessels, in which the red particles of the blood were scarcely at rest, or at least were set in motion by the slightest descent of the compressorium. I have been ready to imagine that the origin of these vesicles in the uterus, and the phenomenon about to be recorded, may be connected with the formation of the blood-vessels and blood. The form of the vesicles in question is not regular; and I have observed an obscure appearance, possibly indicating a communication between their cavities and a more internal part.

282. Being engaged in laying open for examination the oviduct of a Rabbit, I found a little mass adherent to the instrument, which on being viewed in the microscope was found to be a detached portion of its inner membrane. Imbedded in this little mass was a vesicle (Plate IX. fig. 151.) of an ellipsoidal form, $\frac{1}{7}'''$ in length, consisting of a tolerably thick membrane, having on its inner surface a layer of elliptic granules (vesicles?), and containing a pellucid fluid. This being apparently one of the vesicles in question, would not perhaps have been examined farther, but for a remarkable phenomenon observed in its interior.

283. In the centre of the fluid of the vesicle there was an object (see the figure) resembling the mulberry-like structure, which I have described in the mammiferous ovum (Plate VI. figs. 109 to 112.), actually *revolving on its own axis*. The revolutions were in the vertical plane, a direction which was not observed to change. In point of time they were not quite regular, being sometimes rapid, while at other times the object seemed nearly, though not quite at rest. Twice the revolving body was observed suddenly to shift its situation in the fluid, make a short circuit, and then almost immediately return to the centre of the vesicle. It had a diameter of $\frac{1}{50}'''$. Granules (vesicles?) were occasionally seen to start from their situation in the layer lining the membrane of the vesicle, and move towards the centre of the fluid; but they were not observed to attach themselves to the revolving body.

284. The object was watched revolving for half an hour, at the end of which time its revolutions terminated rather suddenly. The little mass then for a few seconds seemed to be at rest. It next assumed a tremulous motion, which—interrupted for a few seconds by a renewal of the revolutions—continued for about fifteen minutes, making three quarters of an hour in all, during which the object was observed to be in motion. And it is deserving of remark, that the Rabbit had been killed an hour and half before the examination was begun. Having ceased to move, the little mass was broken up by the compressor, when some of its parts, still hanging together, presented a renewal of the tremulous motion just mentioned. During these observations ciliary motions were very vivid on the mucous membrane investing the vesicle in question. They had become languid when the revolutions ceased. No cilia were observed on the revolving object itself; but cilia may have been present, and from their minuteness invisible†.

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† On the subject of cilia as a cause of rotatory motion, I refer to Professor SHARPEY's excellent paper in Dr. TODD's "Cyclopædia of Anatomy and Physiology," vol. i. p. 606.

285. I cannot better describe the appearance of the object now referred to as revolving on its own axis, than by stating that it would not have been easy to distinguish it from the mulberry-like structure (Plate VI. figs. 109 to 112.) occupying in certain stages the centre of the mammiferous ovum, and containing the structure which I consider to be the primordial germ. The revolving object in the vesicle (Plate IX. fig. 151.) was however smaller. The vesicles (fig. 152.) composing the surface of the mulberry-like structure in these vesicles vary in their size, many of them measuring $\frac{1}{300}'''$. Among them I have repeatedly observed several globules, or rather vesicles, which appeared to have occupied the centre of the mass. They were larger than those at the surface, granulous, yellowish-brown in colour, and contained themselves a globule. In some instances I have observed the whole mulberry to be tinged yellowish-brown, the colour being deepest in the centre of its vesicles. The vesicles, it will be observed (fig. 152.), contain objects (nuclei?) in their interior.

286. The phenomenon of *rotation* of the central mass in these vesicles I have seen only once, but it has not been particularly sought for. Frequently, however, has the existence of such a mass been observed with the same mulberry-like form. And in repeated instances the vesicles of the mass, more or less separated, have exhibited the tremulous motion above mentioned. In one instance the motion was comparable to that observed in separate globules when in the neighbourhood of cilia in motion.

287. It has been remarked by Professor BURDACH[†], that "the mammiferous ovum, not only in its form, but also in its vital relations, resembles a hydatid." The resemblance, however, was probably not conjectured to be so very close as from the above facts it appears to be. (Compare Plate IX. fig. 151. with several of the figures in Plate VI.) It remains to be discovered whether the mulberry-like structure with its germ in the ovum of Mammalia, also performs rotatory motions. The thought that it may do so has naturally suggested itself, from the striking resemblance in form, composition, and locality[‡] of that structure in the mammiferous ovum and the uterine vesicles in question.

288. It is known that the *embryos* of a number of Mollusca, as well as the *germinal granules* of some Polypes, &c., perform rotatory motions. We are indebted to Professor GRANT for some very interesting facts on this subject. Several observers have noticed this phenomenon in the *ova*, or *yelk-ball*, of certain animals. An instance of its occurrence in the latter appears to have been observed by LEEUWENHOEK in 1695, but the subject claimed little attention until revived by CARUS and E. H. WEBER. CARUS[§] has investigated the subject in Mollusca. With reference chiefly to the ob-

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[†] *L. c.*, vol. ii. p. 820.

[‡] It is not unusual to meet with vesicles in which the object in question appears to have left the centre of the cavity, and approached the membrane on one side (par. 185.).

[§] Von den äussern Lebensbedingungen der weiss- und kaltblütigen Thiere, 1823; and Neue Untersuchungen über die Entwickelungsgeschichte unserer Flussmuschel, 1832.

servations of the last-named author, Burdach[†] states the following regarding the ova of *Limnæus stagnalis*. "With the incipient formation and development of an embryo, the yelk-ball performs in the vertical plane [par. 283.] rotatory motions around its axis, the cause of which has not been satisfactorily explained. The yelk granules *swell*, and become large and *vesicular*. After some days the revolutions become weaker." Is it not possible, that the so-called "yelk" in this instance, the granules of which became "*vesicular*," really corresponded to the mulberry-like structure I have met with in the ovum of Mammalia and in the vesicles just mentioned? Carus[‡] has mentioned rotatory motions as occurring in the ova of *Unio tumida*. These were horizontal, and accompanied by a change in the form of the yelk. The same observer saw in *Anodonta intermedia* corresponding motions, which were subject to temporary cessation, and the direction of which was irregular.

289. It is an interesting fact, that in two out of the three genera above mentioned —as affording examples of revolving ova—there has been observed a structure that appears to be essentially the same as that which I have met with in the ovum and the uterine vesicles in question of Mammalia, producing the appearance of a mulberry.

Correspondence in the Elementary Structure of Animals and Plants.

290. Few microscopic investigations have led to discoveries so interesting and important as those of Schwann[§] on the correspondence in the structure and growth of animals and plants; and certainly no stronger proof can be required of the importance of the history of development to physiology. That author, basing his researches in the animal upon the discoveries of Schleiden[‖] in the vegetable kingdom, has demonstrated that in development the same phenomena are exhibited in both. He has shown not only that animal tissues in general, like those of plants, are reducible to modifications of vesicles, or as he calls them "cells,"—but that the mode of origin of the vesicles or "cells" is essentially the same in animals as Schleiden had discovered it to be in plants. The membrane of each vesicle or "cell" is formed at the surface of a previously existing nucleus, which is a minute, spherical, or elliptical, and often flattened body, having a granulous appearance, and found by Schleiden in many instances to contain a nucleolus[¶]. The membrane of the vesicle was found by Schleiden to rise from the nucleus, and in the early progress of distention to present an appearance which he compares to that of a watch-glass on a watch. The analogy now mentioned extends to organized as well as unorganized animal tissues; and even the blood-vessels are formed of vesicles or "cells." Farther, elementary parts, which in a physiological point of view are entirely different, have been shown by Schwann to

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[†] *L. c.*, vol. ii. p. 224. [‡] Neue Untersuchungen, &c. [§] Mikroskopische Untersuchungen, &c.

[‖] "Beiträge zur Phytogenesis," in Müller's Archiv, 1838. Heft II.

[¶] The nucleus had been known to other botanists, its importance having been first conceived by our very distinguished countryman Robert Brown; but its property of originating the "cell" was the discovery of Schleiden, who from this property proposes to denominate it the "*Cytoblast*."

follow the same laws in their development. Thus whether a muscular fibre or a nervous tube is destined to be formed, the foundation of both consists of vesicles or "cells," which have arisen in the manner above described; and it is through the modifications which the vesicles undergo, that a muscular fibre on the one hand, or a nervous tube on the other, is produced. "In short, there is for all the elementary parts of organisms a common principle of development."

291. It will be interesting to refer to a few of the facts recorded in the foregoing and the previous memoir, in connexion with the analogy now mentioned; and if those facts should be found in any way to exemplify it, they will not be the less admissible from my having observed them in the course of researches in which this analogy formed no part of the object I was in pursuit of.

292. Both the nucleus and nucleolus were figured in my "First Series[†]," in the peculiar granules, or rather vesicles of the ovisac, though I was ignorant of their importance. It will be seen indeed that those peculiar granules (vesicles) form a part of more than fifty figures in that paper, and that the nucleus is represented in almost every instance where the size admitted of it. This is mentioned merely to show that those objects (the nucleus and nucleolus) were not overlooked, for to SCHLEIDEN belongs the merit of first pointing out the nature of those objects in plants, and to SCHWANN that of recognising corresponding structures in animals. That memoir contained also the drawing of a spot[‡] on the inner surface of the membrane of the yelk in the ovum of the Frog. Of that spot I did not attempt to offer any explanation, simply stating its appearance as "a spot which I always find on the internal surface of the membrana vitelli of the Frog in ovisacs of about this size [$\frac{1}{3}'''$]. This spot does not appear to have been hitherto described. It is generally elliptic, rarely round, has a well-defined contour, and is perhaps slightly lenticular in form. In this instance it measured $\frac{1}{83}'''$ in length, and is often of about the same size. It appears to be composed of granules[§]." The spot in question is obviously the nucleus of the membrane or vesicle on the inner surface of which it occurs[‖].

293. The vesicles which constitute the outer portion of the mulberry-like structure (Plate VI. fig. 110., Plate VIII. fig. 130.) present each a nucleus[¶] and nucleolus.

[†] *L. c.*, Plate VIII. fig. 73.

[‡] *L. c.*, Plate VI. fig. 28. *d'*, and in the present paper, Plate V. fig. 84½. *x*.

[§] *L. c.*, par. 40.

[‖] The existence of this spot will probably be found of some importance in determining the order of formation of the several parts of which the ovum is composed. And it is proper to state, that SCHLEIDEN's discovery in plants of the origin of vesicles ("cells") on a nucleus, and the extension of this discovery to animals by SCHWANN, must necessarily modify some of the conclusions in my "First Series" as to the order of formation here referred to; but in the present series this has not been the subject of investigation. I would remark, however, that if the spot on the inner surface of the membrana vitelli in the ovum of the Frog is present in the corresponding membrane of other ova, the formation of this membrane is doubtless earlier than that of the germinal vesicle itself (par. 242.). I have observed the stroma in the neighbourhood of an ovisac to appear as if composed of vesicles with nuclei.

[¶] It is not probable, however, that the nucleus here referred to is of the same kind as that called the "Cytoblast" by SCHLEIDEN (see par. 317. and its last Note.).

294. The vesicles composing the layer (*am.*) on the inner surface of the membrane *f* (Plate VI. figs. 111 to 117., Plate VIII. fig. 129) have each their nucleus[‡] and nucleolus. Hence the amnion—formed as it is out of these vesicles—may be added to the structures already found to be referable to a "cell"-formation.

295. The lamina in which I suppose the blood and blood-vessels to form, presented in its mode of origin a series of changes which do not seem to have been before observed in any animal, and it will be seen that they are most intimately connected with the present subject. (See par. 196. 197. 201 to 204. 211.).

296. The yelk-globules are true vesicles, containing other vesicles (Plate V. fig. 87.). The villi of the chorion are vesicles in which I observed objects having the appearance of vesicles (Plate VIII. fig. 141.). The whole embryo indeed is composed of vesicles (Plate VIII. figs. 121 A. 121 B. 122. bb^1. and bb^2.); and *even the primordial germ itself* (Plate VI. fig. 113. *bb.*) seems to have been the nucleus of the vesicle in the centre of which it lies[†].

297. I have now to mention a fact or two regarding the nature and properties of nuclei. Schleiden supposes the nucleolus to exist before the nucleus, and Schwann believes that he has observed the formation of the nucleus to take place around the nucleolus. Hence the last-mentioned observer considers that it may be said, "the formation of the cell [vesicle] is only a repetition around the nucleus, of the same process through which the nucleus was formed around the nucleolus;" the difference being only in degree. In connexion with this view Schwann refers to the fact, that nuclei often become hollow vesicles. On the period of origin of the nucleolus I have no observations, except that its existence was not appreciable in any of the nuclei represented in Plate VII. figs. 120 and 121[‡]., and not in all of those in Plate VIII. fig. 150[‡]. Several observations, however, enable me to state that objects occupying the situation of nuclei are sometimes hollow vesicles. Such for instance was their condition in the amnion (Plate VIII. figs. 129. 130[‡].) and in the lamina subsequently vascular (fig. 150[‡].). I have also observed that with incipient decomposition of the peculiar granules (vesicles) of the ovisac, their nuclei appear like vesicles, filled with a colourless and pellucid fluid (Plate V. fig. 102. *g.*). Whether this results from distention of the nucleolus (frequently seen to be hollow), or from liquefaction of the contents of the nucleus including the nucleolus, I do not know.

299. The following observation (before noticed) may serve to extend our knowledge as to the properties of a nucleus. If Plate V. fig. 89. *b.* be referred to, it will be seen that the germinal spot (as such) has disappeared, and that in its stead are several vesicles with intermediate granules[§]. From those vesicles the germinal vesicle (*c.*) did not appear to differ except in its greater size. This was the effect of incipient decomposition. Here then the germinal spot (possibly, as supposed by Schwann, a

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[†] See the first part of the Note to par. 186.

[‡] See the Note to par. 293.

[§] In the ovum Plate V. fig. 86. the same phenomenon was observed, though with less distinctness.

nucleus in reference to the germinal vesicle) was seen to have resolved itself into vesicles; a fact, which seems deserving of notice, as being the result of decomposition.

300. The primordial germ, as already stated, seems to be a nucleus. And here we find the most important of all nuclei resolving itself entirely into vesicles,—the first of these vesicles containing that which I have denominated the peripheral portion of the germ (Plate VIII. fig. 148. bb^2.). (See the *primitive* changes in the germ par. 209. 212 to 215. 312. and Plate VII. figs. 121 A. 121 B. 122.)†.

301. The vesicles exhibited in Plate VIII. fig. 149. constituted part of the incipient embryo in an ovum of 111½ hours and measuring ⅕'''. They were drawn after removal from their situation, and after the ovum had lain forty-eight hours in kreosote water. It will be seen that those vesicles contained two or even three minuter vesicles, inclosed the one within the other. The arrangement was not concentric, all the vesicles being in contact at a certain point, while on the other side there were intervening spaces. This was observed to be the case with the vesicles into which the germinal spot, above referred to, had resolved itself (Plate V. fig. 89. *b*.) from incipient decomposition.

302. In these researches no objects have so frequently been met with as those which I have called "dark globules." They present themselves with the earliest appearance of the ovum (Plate V. figs. 81 to 83.), and in many stages of its development. Wherever a part is beginning to be formed, or the formation is vigorously proceeding, there are seen dark globules. The yelk, the peculiar granules (vesicles) of the ovisac, and the villi of the chorion afford examples. They are present in very large quantity, among the vesicles of which the incipient embryo is composed (Plate VIII. fig. 149.); and here they appear to be no other than the foundations of new vesicles. (Compare the inner with the outer part of bb^1 in Plate VII. fig. 121 A.) I am disposed to think that this is the case in many other instances. For example, the vesicles destined to form the villi of the chorion arise in this manner (par. 223.); and the dark globules of the yelk we have seen to be true vesicles containing other vesicles (par. 121. and Plate V. fig. 87.).

303. The space between the membrane of an outer and the membrane of an inner vesicle (or nucleus) seems in many instances to be the place of origin of dark globules, or incipient vesicles‡. During the *primitive* changes in the germ (par. 312.)

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† The resemblance in position and appearance between the germ (Plate VI. fig. 113. *bb*.) and the germinal spot after impregnation (Plate V. figs. 93 and 96 *b*.) is very remarkable. So great is this resemblance that at first I supposed and recorded it to be the germinal spot and vesicle again coming into view. And it has been only in consequence of my inability to discover these objects in intermediate stages (such as those represented in Plate VI. fig. 105½ to 108.) that I have been induced to give up the opinion that the germinal spot is identical with that which I have called the germ; and to maintain (as I am compelled to do) that the germ is a new formation, not originating until after the ovum has been fecundated. The resemblance between the subsequent appearance of the germ, as composed of vesicles (Plate VII. figs. 121 A. to 122.), and the condition of the germinal spot when undergoing decomposition (Plate V. fig. 89. *b*.), is not less remarkable. (See, however, the first part of the Note to par. 186, and the Notes to par. 193 and 197.)

‡ In plants, according to SCHLEIDEN, the origin of vesicles within already existing vesicles is nearly or quite

an example of this occurs (Plate VII. figs. 121 A. to 123. bb^1.). Each of the concentric layers of which the germ consists, is so distinctly circumscribed as to appear delicately membranous at its surface (par. 212.); or in other words, each concentric layer appears to be contained within a vesicle. Now here, as just stated, the intervening spaces are occupied by dark globules, subsequently forming vesicles.

304. More particularly considered, the situation, and perhaps the place of origin of the dark globules in question, is in some instances *on the outer surface of the inner vesicle or nucleus*; of which Plate VIII. fig. 150. affords a remarkable example, in the lamina subsequently vascular[†]. The dark globules in the network Plate VIII. fig. 132. in every instance surround the nucleus[†]. The yelk-globules (vesicles) seem to arise on the outer surface of the germinal vesicle[‡][†]. In other instances the foundations of new vesicles arise as dark globules on the external surface of an *outer* vesicle. This is the case with the vesicles forming the villi of the chorion (par. 223.); and perhaps the "isolated spots" occurring in the Graafian vesicle may be mentioned as another example[§].

305. On the liquefaction of the membrane of a vesicle dark globules are sometimes liberated. This takes place, for instance, when the membranous portion of the network (composed of vesicles, par. 201.) Plate VIII. fig. 132. disappears. (See Plate VII. fig. 120. Whether the dark globules which the latter figure shows to have been set free, are the foundations of new vesicles, I do not know; but in an ovum of about the same size the corresponding part presented vesicles in such quantity as to be pressed together into polyhedral figures, Plate VIII. fig. 150. From some observations on the vesicles of which the incipient embryo is composed, I am disposed to think the dark globules exhibited in Plate VIII. fig. 149. (or a part of them) may have been liberated in the same manner.

306. In Plate VIII. fig. 143. is an ovum of twenty-three hours from the Fallopian tube. It was found with five others, and they were all lying very near together. These ova had penetrated about $\frac{1}{4}$ to $\frac{1}{2}$ an inch into that part of the tube where its calibre suddenly diminishes; a part which when rolled between the fingers is found to resemble the vas deferens. In this ovum (fig. 143.) the chorion had not separated

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universal. In the formation of animal tissues, on the other hand, SCHWANN found this to be rarely the case. The observations, however, now referred to, of the origin of vesicles in vesicles, relate to a period anterior to the formation of even the elementary parts of tissues (nervous tubes, muscular fibres, &c.), and therefore are not necessarily opposed to those of SCHWANN.

[†] See the Note to par. 293.

[‡] See my "First Series," *l. c.*, Plate V. figs. 14 and 16. It is interesting to find, as already stated, not only that the *formation*, but also that the *liquefaction* of the yelk-globules (vesicles) begins around the germinal vesicle (Plate V. figs. 99 and 100.).

[§] These "isolated spots" were described in my "First Series" (par. 59. Plate VIII. fig. 66.) as consisting "of one of the peculiar granules [vesicles] of the Graafian vesicle, having a peripheral accumulation consisting of oil-like globules."

from the membrane *f*, but it was visible as a dark line surrounding the latter. (See also Plate VI. fig. 104. *a*.) The membrane *f* contained two objects measuring respectively $\frac{1}{33}'''$ and $\frac{1}{38}'''$ in length†. Those objects had the appearance of two yelk-balls, and I am by no means sure that this was not the case. It seems possible, however, that they arose from a division of the previous yelk-ball into two portions. If so, this ovum was in a stage between that represented in Plate VI. figs. 105 and 106‡. The resemblance it bears to the ovum of Batrachian Reptiles after the first division of the yelk has taken place is remarkable§, and favours the latter supposition. Having met with but a single ovum in this state, however, I must leave this question for future observation to decide. In each of the two objects present in the ovum now referred to (Plate VIII. fig. 143.) there was observed a brilliant point.

307. The stages of the mammiferous ovum, exhibited in Plate VIII. figs. 106 to 108. presenting as they do successive sets of vesicles, each set consisting of smaller and more numerous vesicles than the last, suggest the idea that in the interior of each vesicle there arise two or more infant vesicles, the parent vesicle in each instance disappearing by liquefaction. Those later stages also bring to mind the divisions and subdivisions first observed by Prevost and Dumas in the ovum of Batrachian Reptiles, and since found by Rusconi to occur in some osseous Fishes; suggesting also that these divisions and subdivisions may be referable to the formation of vesicles‖. Rusconi observed in the ovum of the Frog that some of the masses

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† The *yelk-ball* in the other ova found at the same time had a diameter in one instance of $\frac{1}{16}'''$, and in another of $\frac{1}{17}'''$.

‡ It will be observed that the ovum Plate VIII. fig. 143. is exhibited on a larger scale than the ova in Plate VI. fig. 105 and 106.

§ Compare Plate VIII. fig. 143. with figures by Rusconi of the ovum of the Salamander and the Frog (Müller's Archiv, 1836. Heft II. Tab. VIII.).

‖ Schwann, without knowing of the formation of the vesicles which I have described in the centre of the mammiferous ovum, conjectured that possibly the divisions in the ovum of the Frog, &c. might be reducible to a formation of vesicles; "two vesicles first developing themselves within the yelk, in each of the same two new vesicles, and so on." (*l. c.*, pp. 61. 62.). Bischoff after observing the yelk to have, as he supposed, an "angular" appearance in the ovum of the Dog, proceeds, "I have sometimes seen forms so remarkable, that the thought occurred to me, whether the yelk of the mammiferous ovum also undergoes changes in form similar to those in the ova of Fishes and Batrachians. But unfortunately these forms of the yelk are very soon lost, as soon as the ovum is placed in water or saliva, probably through imbibition of this fluid. Hence I have not been able to note some of them." (*l. c.*, p. 96.) It appears to me, that what Bischoff here terms "remarkable forms of the yelk" must have corresponded to my mulberry-like structure in its different stages.—That observer concludes the account of his researches with the following remarks. "Putting together my more recent with the earlier observations, I think the process of the formation of cells is the following: the yelk-granules first group themselves in smaller globular masses [nuclei], which then become surrounded by a cell. With the growth of the cell the granules separate from one another, group themselves in concentric rings, and now the nuclei become new cells, until at last each yelk-granule is surrounded by a cell, and the germinal membrane is so formed as in figs. VII. and VIII., where each cell contains a simple nucleus. In figs. IV. and V. the cells are still few, and hence there are many granules in each; in fig. VI. there are already more vesicles and the granules are fewer; in figs. VII. and VIII. the cells are still more numerous and each contains only one (vesicles?) resulting from the divisions and subdivisions, presented cavities in their interior. Von Baer[†] and Rusconi[‡] have both figured a flattened "cavity" in the ovum of the Frog on that side where the divisions and subdivisions (i. e. the formation of vesicles?) had most rapidly proceeded. Does this "cavity" correspond to my vesicle which contains the germ[§]?

Anomalous Ova.

308. The object represented in Plate VIII. fig. 144. was found with four ova in the uterus about $\frac{3}{4}$ inch from the Fallopian tube. The period was $111\frac{1}{2}$ hours. The ova measured about $\frac{1}{3}'''$; the object in question $\frac{1}{11}'''$. The latter consisted of the following parts; viz. an outer membrane (*f*), resembling the thick transparent membrane ("zona pellucida") of the ovarian ovum; an inner pear-shaped membrane (*e*), $\frac{1}{16}'''$ in length, having considerable thickness; and in the interior of this second membrane, two vesicles, one of which measured $\frac{1}{56}'''$, the other $\frac{1}{36}'''$. The membranes of these two vesicles also had considerable thickness. All the membranes were transparent, and contained a colourless transparent fluid. The two smallest vesicles presented a mass in their centres having a granulous appearance. I am disposed to think this object was an ovum, but it is not easy to determine the nature of its parts[||]. On another occasion I found in the uterus an object of $\frac{1}{8}'''$ resembling the above, but the second membrane (*e*) was not present.

The origin of Tissues subordinate to a division into forms.

312. It will be remembered that the germ when first seen has the appearance of a granulous thick-walled hollow sphere, containing a brightly pellucid fluid (Plate VI. fig. 113. *bb*.), and that this object having separated into two portions (Plate VIII. fig. 148. bb^1 and bb^2.), the central portion undergoes the following changes[¶]. It presents a pointed process (Plate VII. fig. 118. bb^1.), and resolves itself into vesicles,

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nucleus. If we would extend this process still farther, the changes in form of the yelk are indeed so determined, that in the first place all the yelk-granules are inclosed in two, then in four, then in eight, &c. cells, until each yelk-granule has its cell and *therewith the germinal membrane is formed until the arising of the embryo.* The cells are besides of different sizes; in figs. IV. and V. most of them measured 0,0014 to 0,0018 Par. inch, but some only 0,0008 Par. inch." (*l. c.*, pp. 100, 101.). (See par. 314 to 317.)

[†] Müller's Archiv, 1834, Heft VI.

[‡] *Ib.* 1836, Heft II.

[§] Should the analogy in question really exist, it will prove that Rusconi was correct when he stated, in opposition to the opinion of Baer, "that the furrows in the ovum of the Frog are the *effect* of impregnation, and not the means by which impregnation is facilitated." (Müller's Archiv, 1836, Heft II. p. 208.)—R. Wagner adopts this opinion of Rusconi (Lehrbuch, &c.).

[||] Further observations have satisfied me that this was an ovum aborted in a stage between the conditions represented in Plate VI. figs. 105 and $105\frac{1}{2}$ (see par. 317. *Note.*). The other object above mentioned as measuring $\frac{1}{8}'''$, appears to have been an ovum aborted in the state represented in Plate VI. fig. $105\frac{1}{2}$. In neither of these ova had the chorion arisen.

[¶] See, however, the Notes to par. 193 and 197.

which are disposed in layers (figs. 121 A. to 122 bb^1.). Layer after layer comes into view in the interior (each layer appearing as if invested by a delicate capsule), while those previously occupying that situation are pushed farther out. These changes may be called *primitive*†, for at a certain period no more layers make their appearance, and the cavity inclosed by the most central layer becomes enlarged. It is very interesting to observe, that before those *primitive* changes have reached their limit, a structure is produced (by the layers of vesicles in question) having the *form* of the central portion of the nervous system, including even the sinus rhomboidalis of the spinal cord‡. It is also interesting to find that the foundation of the vascular system is a part of the primordial germ (par. 216.). The nature of the alterations which the germ undergoes immediately after the termination of the primitive changes now referred to, I do not know, not having carried my investigations beyond that period. It is probable that they consist chiefly in the formation of new vesicles. The object represented in figs. 125 and 126.—met with incidentally—was in a much more advanced state as to *form*, and exhibited an incipient division into further forms (par. 218.); yet it presented no trace of separation into tissues. *The origin of tissues, then, is subordinate to a division into forms.* This may serve to elucidate the fact, that parts in different animals, having a general resemblance in *form*, are sometimes the seat of very different *functions*. And if the primordial germs of organized beings in general resemble one another in appearance, as much as, from my observations, it appears that they do in some instances very remote from one another, it is not surprising that there exist resemblances in the subsequent *forms* of beings more nearly allied. I refer, for example, to a curious resemblance between the mammiferous germ with its vesicle—as exhibited in Plate VI. fig. 113.—and the "cytoblast" and spore of a cryptogamous plant (*Rhizina lævigata*),—as figured by SCHLEIDEN in his paper on vegetable structure§. This observation will perhaps be considered as not undeserving of notice in connexion with the subject of analogies in the fundamental structure of organized beings. We find also, that there is a period in the formation of the Mammal —and therefore, it may be presumed, of Man himself—when the immediate foundation of the new being is contained within a simple "cell," vesicle, or sac.

Method of obtaining the minutest Ova in the Uterus and Fallopian Tube; and a mode of preserving them while under examination.

313. The almost entire absence heretofore of observations on the smallest ova in the oviduct of the Mammalia, may be attributed to the difficulty experienced by ob-

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† The separation of the germ into a central and a peripheral portion being the first of these *primitive* changes.

‡ I apprehend it would be an error to suppose that the fluid contained within a tube is the first appearance of the central portion of the nervous system. Its original foundation seems to be present from the first; and its form, as above stated, is seen before that of any other part. The object represented in Plate VII. figs. 125 and 126.—and more particularly its inner part—I am disposed to think consisted chiefly of the foundation of the central portion of the nervous system.

§ *L. c.*, Tab. III. fig. 11.

servers in obtaining objects so minute, and to their inability to apply to those delicate objects—when found—an equally delicate manipulation. My own efforts were for a long while nearly fruitless from the same causes (par. 166.). But experience having at length enabled me to surmount in some degree those difficulties, I will describe the means by which this may be accomplished. Before commencing the examination I provide several discs of glass, of about the diameter and thickness of a small wafer. Some easily compressible *oily* substance—I employ glaziers' putty—is then rolled out into the form of a slender thread, and a portion of it in this state formed into a ring upon each of the glass discs, in such a manner that a closed cavity may be produced in the centre when the disc is inverted and placed upon a plane surface.—The *Uterus*. Having laid open its cavity, and ascertained, by a careful inspection of its walls, that the ova are not visible with the naked eye, or by means of a pocket lens,—I proceed to remove successive portions of the mucus, beginning with the surface nearest to the Fallopian tube. This is done by scraping it off in small quantities with a broad scalpel, the mucus so removed being carefully transferred to a piece of glass. If this glass be held up to the light and examined with a lens, the observer at once detects an ovum, if present, from its peculiar appearance as a pellucid point, distinguishable from a bubble of air or a globule of oil, by the small refraction of light which occurs at its surface, when viewed in such a medium. If the quantity of mucus which lies above the ovum be too great, it may be removed with a very fine hair pencil, care being taken to avoid touching the ovum itself. A disc of glass furnished with its ring of putty above described, is now to be inverted over the mucus, and slight pressure used. A closed cavity is thus made, which is so minute that in a cool atmosphere no sensible evaporation can take place, and the ovum may be viewed for a considerable time in a nearly unchanged state. The object in having the upper glass *thin*, is to admit the use of magnifying powers having a short focal distance. From the yielding nature of the putty, a compressor—if required—is thus ready formed.—The ***Fallopian tube.*** The following passage occurs in the paper, before referred to, of the indefatigable CRUIKSHANK[†]: "Searched in vain for the ova on the right side; at last, by drawing a probe gently over the Fallopian tube on the left side, before it was opened, more than an inch on the side next the uterus, I pressed out several ova, which seemed to come from about its middle, as I began the pressure there, and the ova did not appear till the very last." Yet CRUIKSHANK himself seems not to have applied this method in any other instance, for he subsequently mentions that some of the ova of another Rabbit "were lost from the great difficulty in slitting up the Fallopian tubes without bruising the ova with the fingers or with the point of scissors[‡]." To the casual mention by CRUIKSHANK, of an alternative resorted to by him on one occasion,—which seems to have remained unnoticed for nearly half a century,—I am indebted for a mode of obtaining ova from the Fallopian tube, that—with some modifications—I now always practise; and which, had I used it from the first, would have enabled me to include in the

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[†] *L. c.*, p. 208, Experiment XXIII.

[‡] *L. c.*, p. 208.

Table hereafter given (par. 319.) a much larger number of ova from that narrow passage. I never lay the tube open. Having with fine scissors removed all the adherent substance,—taking care not to apply pressure to the tube at any part,—I extend it in its whole length, divide it in the middle, and proceed to examine the uterine or ovarian half, according to the age of the ova. This is done by cutting off a small portion—say half an inch—of the unopened tube, and pressing out its contents upon a piece of glass with a scalpel. (It is probable that some ova are thus destroyed. But this is of no consequence at all, when we recollect the time saved, and the difficulty in obtaining ova by any other means.) The glass containing the mucus from the portion of Fallopian tube, is then to be held up to the light and examined with a lens as before. If from the minuteness of the ova a lens is insufficient, the mucus may be examined in the microscope with a low power,—say twenty-five diameters,—when the ova if present cannot fail to be detected, whatever their minuteness. It not unfrequently happens that several present themselves in the same field of view. When the mucus is found to contain an ovum, the little disc of glass with its plastic ring should be *instantly* applied, to prevent evaporation†.

SECOND POSTSCRIPT.

Process by which the primitive Ovum undergoes Division and Subdivision.—Probable Analogy in this respect between the Mammiferous Ovum and the Ovum of Batrachian Reptiles and certain Fishes.—The so-called "Yelk-ball" in the Mammiferous Ovum compared with the "Discus vitellinus" in the Ova of other Animals.—Resemblance between the fecundated Ovum of Mammalia and that of certain Plants.

314. In reference to the mode in which the successive groups of vesicles are produced in the centre of the ovum during its passage through the Fallopian tube, I ventured to suggest (par. 307.) "that in the interior of each vesicle there arise two or more infant vesicles, the parent vesicle in each instance disappearing by liquefaction." Such really is the case, as proved by the condition of ova which I have met with since.

315. In Plate VI. fig. 105½ is exhibited an ovum of twenty-four hours (from the Fallopian tube), which in reference to its central part was in a condition between the stages seen in figs. 105 and 106‡. The "yelk-ball" (fig. 105.) having—as such—disappeared, *two* vesicles (fig. 105½.) occupied its place; and in another ovum, instead of two corresponding vesicles, *four* (fig. 106.) were found in the same situation.

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† Care must be taken that the surface of the glass is dry, and that none of the mucus insinuates itself under the putty. I have in more than one instance preserved the ovum in cold weather for a length of time, which far exceeded expectation. The distance between the two glasses should little exceed the diameter of the ovum, by which the included air is reduced to a very minute quantity.

‡ The chorion was less advanced than in the ovum fig. 105.

316. Each of the *two* vesicles in the centre of the ovum (Plate VI. fig. 105½.) was elliptical and flattened, highly transparent, exhibited a very sharp black contour, and contained a transparent fluid, granules, and a spherical pellucid object having a central situation†. A description in many respects the same has been given (par. 174.) of the *four* vesicles in fig. 106.

317. "That in the interior of each vesicle there arise two or more infant vesicles," we have a proof in Plate IX. fig. 155. Here two outer vesicles are seen corresponding to the two which occupied the centre of the ovum Plate VI. fig. 105½‡. The age of the ovum Plate VI. fig. 105½. was 24 hours; that of the one from which Plate IX. fig. 155. was taken, 26¼ hours. In each of the two vesicles occupying the centre of the ovum Plate VI. fig. 105½. there were seen (as already stated) granules and a central pellucid sphere; while each of the corresponding vesicles in Plate IX. fig. 155. presented in its interior formed *vesicles*. There can be no doubt that the ovum, the central part of which is represented in Plate IX. fig. 155, was in a state between the conditions of the ova in Plate VI. figs. 105½ and 106; and it is extremely probable that before the production of the state seen in Plate VI. fig. 106, the membranes of two vesicles, such as the outer ones in Plate IX. fig. 155, usually disappear by liquefaction. This process continued, would produce successively states such as those in Plate VI. figs. 107 and 108§; and perhaps it would not be easy to point out the period of its termination.—The position which the germinal vesicle should be considered to occupy in the history of "cells," remains a question not yet solved. If a "Cytoblast" (par. 290. and its last Note), corresponding to that which I find on the vitellary membrane in the ovum of the Frog, is originally present in the ovum of other animals (par. 292. and Note), the germinal vesicle is obviously an infant "cell" in reference to the vitellary membrane (*f*.). But we have seen that in the mammiferous ovum a membrane (*e*) forms around the "yelk," in the same manner apparently as in other situations a membrane or "cell" forms around its nucleus. Now it is known that the germinal vesicle exists before the granules and globules of the "yelk;" those granules and globules collecting and perhaps forming around the germinal vesicle. If, therefore, the "yelk" be a nucleus to the membrane *e*, the germinal vesicle may, perhaps, be considered a *nucleolus* to the "yelk." This would afford an instance of a "cell" (the germinal vesicle) which had probably arisen from a "cytoblast" (the germinal spot), becoming in its turn a nucleolus for the formation of another "cytoblast" (the yelk), which then originates another "cell" (the mem-

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† These pellucid objects from some cause very soon disappeared, and the two vesicles containing them became comparatively opake. This I have found to occur in many ova of the same period, as well as in ova of the succeeding stages; though no pressure had been applied. The vesicles occupying the centre of ova in a stage approaching that in Plate VI. fig. 108., for instance, at first highly transparent, speedily became opake, and seemed to collapse from a spherical into a polyhedral form.

‡ In some instances the difference in size of these vesicles is considerable.

§ The ingenious speculations of Schwann and Bischoff (par. 307. *Note*.), seem thus in some degree to have been realized.

brane *e*). Within the parent "cell" or membrane *e* new vesicles arise[†]; the interior of each of these in its turn presenting others. How far the process forming these new vesicles resembles that just referred to as appearing to produce the parent "cell" or membrane *e*, must be left for future observation to decide. We cannot but observe, however, a resemblance between the contents of the infant and the parent "cell"; a resemblance the more interesting from the long line of objects through which it seems traceable in direct succession from the ovum[‡].

318. We are now perhaps prepared to realize an analogy already hinted at (pars. 177. 307.), between the early changes which I find to occur in the mammiferous ovum and those previously known to take place in the ovum of Batrachian Reptiles, and some osseous Fishes. The divisions and subdivisions occurring in the ova of these animals, may be effected by a process corresponding to that just described as producing similar changes in the ovum of the Mammal. And if in the former no "outer vesicle" nor pellucid sphere has yet been seen, or suspected, to arise and then disappear between each division, this affords no proof of the non-existence of such objects,—which possibly may yet be discovered. In Fishes the divisions in question do not include the whole yelk-ball, but are confined to a projection on one side[§]. In the Frog those divisions include the whole of the yelk-ball, but they begin, and proceed more vigorously, on one side[‖]. In Mammalia also, as we have seen, the divisions include the whole of the so-called "yelk-ball." This comparison may perhaps assist us in

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[†] The aborted ovum, Plate VIII. fig. 144, exhibited what I apprehend to have been the membrane *e* risen in the above manner (at the surface of the "yelk-ball"), but which did not disappear by liquefaction, and was persistent (considerably thickened) until two vesicles had been formed within it (and likewise thickened), each of these presenting a granulous mass in its interior.

[‡] Where, indeed, does this resemblance terminate? We find that it has by no means disappeared in the "cells" (Plate VIII. figs. 130. 129.) which seem to enter into the formation of the amnion, nor in those of the lamina subsequently vascular (figs. 132. 150.); and an eminent observer—Valentin—has pointed out a resemblance still more remarkable between the ovum and his "Bildungskugeln" in various parts of the nervous system (Ueber den Verlauf, &c., pp. 146, 147. tab. VII. to IX.). The germinal vesicle-like objects in Plate VI. figs. 120. 121., Plate VIII. figs. 129. 130. 132. and 150.—called "nuclei" in the foregoing memoir—I am disposed to think are really *nucleoli*, for the reason above given regarding the germinal vesicle itself; the granules or globules surrounding them (Plate VIII. figs. 130. 150.) being perhaps the remains of a nucleus, in other instances (figs. 129. 150.) dissipated. We find also that in those germinal vesicle-like objects a minute point,—probably a "cytoblast"—was present in some instances, and had been absorbed (?) in others (par. 318. *Note.*).

[§] As observed by Rusconi in the Tench and Bleak (*Cypr. Tinca* and *alburnus*). Müller's Archiv, 1836, Heft III. and IV. Tab. XIII.

[‖] Von Baer, Müller's Archiv, 1834, Heft VI. Tab. XI. Rusconi, Müller's Archiv, 1836, Heft II. Tab. VIII. In connexion with this subject it is interesting to notice the situation of the germinal vesicle, when last seen, in the ovum of the Frog. It lies under the black layer, where—as a flaccid and flattened object—it occupies nearly half of the entire ovum. (See a very mature ovarian ovum of the Frog represented by R. Wagner (Beiträge, &c., Tab. II. fig. 6. *d.*), in which the germinal vesicle had become much enlarged.) Now it is in this part of the ovum of the Frog that the divisions above referred to commence and most vigorously proceed. (Rusconi, Müller's Archiv, 1836, Heft II. Tab. VIII. Baer, Müller's Archiv, 1834, Heft VI. Tab. XI.) See also what has been above stated regarding the projection, and the divisions occurring in this projection only, in the ovum of certain Fishes.

determining what portion it is of the ova of the animals just mentioned which corresponds to the "yelk-ball" in Mammalia. Is not the "discus vitellinus" in the *ovarian* ovum of the Bird the seat of similar divisions†? If so, it will perhaps appear that the so-called "yelk-ball" in the mammiferous ovum corresponds more particularly to the "discus vitellinus" (with its germinal vesicle) in the ovum of the Bird (par. 122. and *Note*. par. 174. and first Note.)‡.

† Do they not occur in the corresponding parts of ova in general?

‡ If the contents of the ovarian vesicle of Baer in Mammalia correspond to no more than the "discus vitellinus" in the ovarian ovum of Birds and other animals, the former will not appear to be relatively so minute as hitherto supposed. As to the difference in form of these two objects, perhaps a globular form of the substance composing the "discus vitellinus" would have been incompatible with its position under the vitellary membrane and with the presence around it—in the ovum of the Bird for instance—of a large quantity of true yelk, provided for a future purpose; while no such provision being required in the ovum of the Mammal, the substance corresponding, as I suppose, to the "discus vitellinus" of other animals fills the vitellary membrane (*f*), and is therefore globular in form. If the analogy in question really exists, the "discus vitellinus" is obviously a nucleus destined to undergo changes like those occurring in the so-called "yelk-ball" of the Mammalia. The round white spot called the "cicatricula" in the Bird's *laid* egg may possibly correspond to my layer of "cells," Plate VI. figs. 111 to 113, lining the vitellary membrane (*f*) in the *uterine* ovum of the Mammal; while my "mulberry-like object," in the same figures, may perhaps be represented in the Bird's *laid* egg by the structure which lies under the "cicatricula," and has been denominated Keimhügel, cumulus proligerus, Kern des Hahnentrittes, nucleus cicatriculæ s. blastodermatis, &c.

In reference to the subject of par. 317. and its last note, it may be added, that my view of the germinal vesicle as a *nucleolus* accords with the experience of Schleiden and of Schwann, that the nucleolus exists before the nucleus (par. 297.). The opinion expressed in the last note to par. 317, that the germinal vesicle-like objects called "nuclei" in the foregoing memoir are in strictness *nucleoli*, is applicable to the corresponding objects in previous conditions of the ovum;—for instance, in such as that exhibited in Plate VI. fig. 105½. Each of the twin "cells" here seen contained a *single* germinal vesicle-like object. In an ovum rather more advanced, but not yet in the state Plate IX. fig. 155, I saw *two* such germinal vesicle-like objects in the same twin "cell." And there seems to be a period in the life of each "cell" (Plate VI. figs. 105½, 106, 107, 108.) when the single germinal vesicle-like object disappears. Do two or more (the nucleoli of new and infant "cells") then arise as its successors? In two instances germinal vesicle-like objects were observed in a state of the ovum resembling that in Plate IX. fig. 155.; and one or two of them presented a dark point, which possibly was a germinal spot-like "cytoblast." Valentin compared an object observed by him in his "germinal vesicle-like nuclei" to the germinal spot. In one instance (Ueber den Verlauf, &c. tab. IX. fig. 73.) he has figured this object as surrounded by a circle. Had the circle been produced by a process of the same kind as that which effected the change in the germinal spot itself, seen in Plate V. figs. 89 *b*. (par. 124. 299.)? The fact that the germinal spot becomes hollow in certain states (Plate V. figs. 89, 93, 97, 102 *b*.) is interesting in connection with the experience of Schwann (regarding other "cytoblasts") referred to in par. 297.—The layer of "cells" lining the membrane *f*, not yet present in the ovum Plate VI. fig. 110, had made its appearance in the ovum fig. 111. Perhaps the "cells" of that layer arose from a continuation of the same process of division referred to in par. 317. (Two germinal vesicle-like objects (nucleoli) it will be remembered (par. 180.) were seen in one of the "cells" fig. 110.) If so, it may be asked,—in connection with what we have seen to take place within the parent cell (the membrane *e*),—does not this process of division admit of a more extended application in the history of "cells," than merely to the "cells" which are the immediate successors of the parent "cell" or membrane *e*? It is extremely interesting to find apparently the same process in operation at a corresponding period in the fecundated *vegetable* ovum. Such at least is the inference I draw from the delineations given by Schleiden (Wiegmann's Archiv, 1837, Tab. VII.; and London and Edinburgh Philosophical Ma-

319. *Ova found in the Fallopian Tube and Uterus of the Rabbit.*

The following Tables may, perhaps, facilitate the discovery of the minuter ova in the Rabbit by affording a general idea of their locality and size at different periods. But they also serve to show that in both of these respects the ova of different individuals are subject to variation, partly occasioned, perhaps, by differences in the degree of advancement in the rut. They farther show that there frequently exist considerable differences in the size and condition of ova destined to constitute the same litter of young; that there is no fixed relation between the size of the minuter ova and the degree of their development or their locality; and lastly that ova are not unfrequently aborted.

Ova found in the Fallopian Tube of the Rabbit.

Hours *post coitum.*	Number of ova found.	Diameter in fractions of a Paris line.	Condition.	Locality.
Hours. 10	2	$\frac{1}{15}$ and $\frac{1}{12}$	Third stage	One inch from the infundibulum.
17	6	$\frac{1}{14}$ to $\frac{1}{10}$	Varying from stage three to the condition represented Plate IX. fig. 153.	Near the middle of the tube.
23	6	$\frac{1}{12}$ $\frac{1}{14}$ to $\frac{1}{12}$	Plate VIII. fig. 143. Various stages...................	Where the tube suddenly diminishes in its calibre.
24	4	$\frac{1}{12}$ to $\frac{1}{11}$	Plate VI. fig. $105\frac{1}{2}$	Middle of the tube.
26	5	$\frac{1}{11}$ to $\frac{1}{10}$	Plate IX. fig. 155. Apparently aborted	Ovarian side of the middle of the tube.
$26\frac{1}{4}$	4	$\frac{1}{10}$ to $\frac{1}{8}$	Plate IX. fig. 155, and Plate VI. fig. $105\frac{1}{2}$	Ovarian side of the middle of the tube.
$34\frac{1}{4}$	13	$\frac{1}{10}$ to $\frac{1}{9}$	Various stages.	From the middle of the tube to within $\frac{1}{2}$ an inch of the uterus.
$35\frac{3}{4}$	6	$\frac{1}{10}$ to $\frac{1}{8}$	Fifth and sixth stages.	Between the middle and the uterine extremity of the tube.
41	3	$\frac{1}{12}$ — to $\frac{1}{12}$	Fourth stage.	Throughout the tube.
$44\frac{1}{4}$	9	about $\frac{1}{8}$	Eighth stage.	Near the middle of the tube.
$47\frac{1}{2}$	7	$\frac{1}{11}$ to $\frac{1}{8}$	The central portion of the ovum in the seventh and eighth stages. The chorion in the fifth stage, and in one instance still less advanced	Uterine side of the middle of the tube.
48	14	$\frac{1}{8}$ to $\frac{1}{7}$	Between eighth and ninth stages	Near the middle of the tube.
63	1	$\frac{1}{7}$	Ninth stage..	One inch from the uterus.
63	4	$\frac{1}{8}$	Fifth stage	Middle of the tube.
$65\frac{1}{2}$	2	$\frac{1}{11}$ and $\frac{1}{10}$	Second stage	Two inches from the uterus.
$68\frac{1}{4}$	7	$\frac{1}{7}$ to $\frac{1}{5}$	Tenth stage......................	$\frac{1}{4}$ inch from the uterus.
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gazine, vol. xii. 1838, Plate 3.) of the incipient "Embryo" in several plants. The resemblance between those delineations and certain states of the mammiferous ovum in the Fallopian tube is very striking; and it is one for observing which I believe no previous opportunity has been afforded. With my drawings Plate VI. figs. 105.

Some of the Ova found in the Uterus of the Rabbit.

Hours *post coitum.*	Number of ova found.	Diameter in fractions of a Paris line.	Condition.	Locality.
Hours. 79½	4	$\frac{1}{7}$	Eleventh stage	Near the tube.
86	9	$\frac{1}{6}$ to $\frac{1}{4}+$	Twelfth and thirteenth stages	Within an inch of the tube.
94½	4	$\frac{1}{6}$ to $\frac{1}{5}$	Twelfth, thirteenth, and fifteenth stages	Near the tube.
95	4	$\frac{1}{6}$ to $\frac{1}{4}+$	Two in the eighteenth stage	Near the tube.
97¾	6	$\frac{1}{10}$, $\frac{1}{9}$ $\frac{1}{7}$ to $\frac{1}{5}$	Aborted Sixteenth and seventeenth stages	Near the tube.
102½	9	$\frac{1}{7}$ to $\frac{1}{4}$	Thirteenth to seventeenth stages	Throughout the uterus.
103	13	$\frac{1}{4}$ to $\frac{2}{5}$	Sixteenth, nineteenth, and other stages	Near the tube.
105	4	$\frac{1}{7}$ to $\frac{1}{5}$	Fourteenth, and other stages	¾ inch from the tube.
107½	9	$\frac{1}{4}$ to $\frac{3}{4}$	Twenty-first, and other stages	All within one inch of the tube.
107½	6	$\frac{1}{8}$ $\frac{1}{5}-$ to $\frac{1}{3}$	Probably aborted Twentieth, and other stages	Near the tube.
108½	10	$\frac{1}{4}+$ to $\frac{1}{3}+$	Nineteenth, and other stages	Throughout the uterus.
111½	8	$\frac{1}{4}+$ to $\frac{1}{3}$	Twenty-first stage	Near the tube.
112¾	1	$\frac{1}{3}$	?	Near the tube.
114¼	7	$\frac{1}{4}+$ to $\frac{1}{3}$	Twenty-first, and other stages	Near the tube.
114¼	9	$\frac{1}{7}$ to $\frac{1}{4}$	Apparently aborted	Throughout the uterus.
115½	5	$\frac{1}{7}$ to $\frac{1}{5}$	Aborted	Near the tube.
115½	9	$\frac{1}{11}$ $\frac{1}{5}-$ to $\frac{1}{3}$	Aborted. See Plate VIII. fig. 144 Various stages	Near the tube.
115¾	9	$\frac{6}{10}$ to $\frac{7}{10}$	Some in nineteenth stage, others much more advanced	Throughout the uterus.
118	7	$\frac{1}{7}$ $\frac{1}{5}$ to $\frac{5}{11}$	Apparently aborted Various stages	Throughout the uterus.
119	10	$\frac{2}{5}$ to $\frac{3}{4}$	Various stages	Throughout the uterus.
120½	5	$\frac{1}{7}$ to $\frac{1}{6}$	Aborted	Upper half of uterus.
124¼	6	1 to 1$\frac{1}{4}$	Stages later than the twenty-first	Upper half of uterus.
131¾	9	$\frac{1}{8}$ and $\frac{1}{5}$ $\frac{3}{5}$ to 1	Probably aborted Various stages	Upper half of uterus.
	163			

105½. 107. 108. respectively. I would compare those of SCHLEIDEN figs. 14. 15. (*Œnothera crassipes*) figs. 6. 7. (*Potamogeton lucens*). My figure Plate VI. fig. 105½. shows the foundation of the mammiferous embryo to consist at a certain period of two "cells." According to SCHLEIDEN's representation (*l. c.* fig. 15.) such is the case in *Œnothera crassipes*; and here the two "cells" have the same form and general appearance as in the ovum of Mammalia, though germinal vesicle-like objects are not present. The evanescent nature of the latter (already mentioned) may possibly explain their absence in the vegetable figure. Some of my delineations of stages rather more advanced may perhaps admit of comparison with those of SCHLEIDEN, in which the "terminal shoots" (*punctum vegetationis* of WOLFF) come into view, and the cotyledons begin to make their appearance. The granulous appearance in the interior of the "cell" *e* in certain stages, and in that of its

320. *Table of Measurements.*

The measurements are given in fractions of a Paris line ('''), the micrometer used, one of FRAUENHOFER's, being divided according to French measure. The French inch (of twelve lines) is to the English inch, as 1·06575 is to 1·00000, or nearly one fifteenth more. Assuming it to be exactly one fifteenth more, the simplest mode of converting the fraction of a French *line* into the fraction of an English *inch*, will be to multiply the denominator of the former by the number 11·25 (or $11\frac{1}{4}$). Thus the actual length of the embryo in figs. 124. 125. and 126. which measured $\frac{1}{7}$ of a Paris line, is found to have been about $\frac{1}{79}$ of an English inch.

When the object is not spherical, it is the long diameter, the measurement of which is given in the Table.

No. of Figure.	Diameters magnified.	Actual Diameters.					
		bb'. Central portion of the Germ.	*bb²*. Peripheral portion of the Germ.	Embryo *bb'*. + *bb²*.	Mulberry-like Structure.	Membrane *f*.	*Cho.* Chorion.
103 *a*	75	………	………	………	………	$\frac{1}{12}$	
104 *a*	75	………	………	………	………	………	$\frac{1}{12}$
105	75	………	………	………	………	………	$\frac{1}{10}$
$105\frac{1}{2}$	75	………	………	………	………	………	$\frac{1}{11}$
106	75	………	………	………	………	………	$\frac{1}{6}$
109	75	………	………	………	$\frac{1}{23}$	………	$\frac{1}{7}$
110	75	………	………	………	$\frac{1}{20}$	$\frac{1}{12}$	$\frac{1}{5}$
111	75	………	………	………	………	………	$\frac{1}{7}$
112	75	………	………	………	………	$\frac{1}{10}$	$\frac{1}{5}$ +
113	75	………	………	………	………	$\frac{1}{12}$	$\frac{1}{7}$
114	50	………	………	………	………	………	$\frac{1}{4}$ −
115	50	………	………	………	………	………	$\frac{1}{6}$
116	50	………	………	………	………	………	$\frac{1}{3}$
117	50	………	………	………	………	………	$\frac{1}{6}$
118	36	………	………	………	………	$\frac{1}{6}$	$\frac{1}{4}$
119	36	………	………	………	………	………	$\frac{1}{4}$ +
121 A	50	………	$\frac{1}{10}$	………	………	………	$\frac{2}{3}$
122	40	about $\frac{1}{4}$					
123	40	$\frac{1}{4}$					
124	40	………	………	$\frac{1}{7}$	………	………	$\frac{1}{5}$ −
125	120	………	………	$\frac{1}{7}$			
126	120	………	………	$\frac{1}{7}$			
153	75	………	………	………	………	………	about $\frac{1}{10}$

successors (Plate VI. figs. $105\frac{1}{2}$ to 108, &c.), is produced by myriads of minuter and epithelium-like "cells" with which they are filled, as will be shown in drawings on a larger scale to accompany a future paper.

321. Explanation of the Plates.

N.B. In all the figures the same letters denote the same objects; as may be seen by the explanation at the foot of each Plate. The same letters are used as in the "First Series."

b. Germinal spot.
bb. Germ.
  bb^1. Central portion of the germ } Embryo.
  bb^2. Peripheral portion of the germ—"tache embryonnaire" —"area vasculosa."
    $bb^{2'}$. Future vascular lamina of the umbilical vesicle.
c. Germinal vesicle.
d. Yelk,—yelk-globules (vesicles),—escaped yelk.
e. Membrane originally investing the yelk—proper membrane of the yelk (which disappears. Compare in Plate VI. fig. 105. with fig. 105½.),—also yelk-ball.
f. Thick transparent membrane of the ovarian ovum—"zona pellucida."
f^1. Fluid imbibed by the chorion.
g. Peculiar granules (vesicles) of the ovisac.
  g^1. Tunica granulosa.
  g^2. Retinacula.
h. Ovisac.
i. Proper covering of the ovisac,—also *corpus luteum*.
  hi. Graafian vesicle (consisting of *h* + *i*).
k. Stroma.
l. Peritoneum.
x. Nucleus—"cytoblast."
am. Amnion.
  am. f. Union of the membranes *am*. and *f*.
a. p. Area pellucida.
cho. Chorion, becoming villous in the uterus—villous chorion.
  cho^1. Villi.

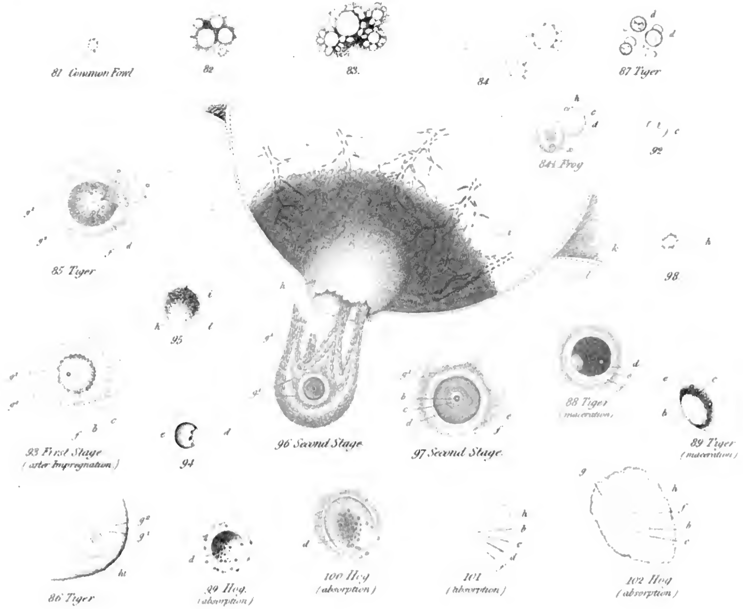
PLATE V.

Fig. 81. Common Fowl (***Phasianus Gallus***, LINN.). A very early stage in the formation of the ovum (par. 242.).

Fig. 82. Rabbit (*Lepus Cuniculus*, LINN.). An extremely early stage in the formation of the ovum. Largest vesicle $\frac{1}{160}'''$ (par. 242.).

Fig. 83. Rabbit. A stage in the formation of the ovum rather more advanced than that in the preceding figure. Largest vesicle $\frac{1}{71}'''$ (par. 242.).

Embryology.



81 Common Fowl

82

83.

84

87 Tiger

84† Frog

92

85 Tiger

98

95

93 First Stage (after Impregnation)

94

96 Second Stage

97 Second Stage

88 Tiger (maceration)

89 Tiger (maceration)

86 Tiger

99 Hog (absorption)

100 Hog (absorption)

101 (absorption)

102 Hog (absorption)

(Where the name of the animal is not given the Figure is from the Rabbit.)

b Germinal Spot
c Germinal Vesicle
d Yelk _ Yelk globules (vesicles)
e Membrane originally investing the Yelk _ Yelk ball
f Thick transparent membrane of the ovarian ovum "Zona pellucida"
g Peculiar Granules (vesicles) of the Ovisac
g¹ Tunica granulosa
g² Retinacula
h Ovisac
i. Proper covering of the Ovisac _ Corpus luteum
hi Graafian vesicle
k Stroma
l. Peritoneum
x Nucleus

Martin Barry

Fig. 84. Rabbit. The stage succeeding that in the last figure. Largest vesicle $\frac{1}{50}'''$. (par. 242.).

Fig. 84½. Frog (*Rana temporaria*, LINN.). Nucleus (x) on the inner surface of the vitellary membrane. *h*. The ovisac (par. 292.)

Fig. 85. Tiger (*Felis Tigris*, LINN.). An immature ovum (par. 120.) crushed to show the nature of the membrane *f*.

Fig. 86. Tiger. Part of a Graafian vesicle (*hi*) containing an ovum with its tunica granulosa (g^1) and retinacula (g^2). The germinal vesicle has become elliptical by maceration for a short time (par. 124.).

Fig. 87. Tiger. Vesicles of the yelk; the largest $\frac{1}{360}'''$. They contain vesicles in their interior (par. 121. 296.).

Fig. 88. Tiger. An ovum after maceration for a short time (par. 123. 124.). The membrane *f* has become distended, and it has imbibed fluid, which lies between that membrane and the proper membrane of the yelk (*e*). The germinal vesicle and germinal spot are altered by maceration. (See fig. 89.)

Fig. 89. Tiger. The germinal vesicle (*c*) and germinal spot (*b*) of the ovum in the last figure, more highly magnified, to show the effects of maceration for a short time. The germinal vesicle has become elliptical, and the germinal spot has resolved itself into several vesicles with intervening granules (par. 124. 299.).

Fig. 92. Rabbit. The germinal vesicle from a fecundated ovum. The vesicle though ruptured has not collapsed, owing to its now consisting of two membranes (par. 133.).

Fig. 93. Rabbit. An ovum from the ovary in the "first stage" of development. (par. 141.).

Fig. 94. Rabbit. The proper membrane of the yelk ($\frac{1}{20}'''$) from an ovum which seemed on the point of being discharged from the ovary. It has thickened (par. 136.).

Fig. 95. Rabbit. A Graafian vesicle 9¼ hours *post coitum*, about to discharge its ovum. The covering (*i*) of the ovisac is now a thick and highly vascular mass; it being this structure which becomes the corpus luteum (par. 152 to 157.).

Fig. 96. Rabbit. The same on a larger scale, and after being ruptured (par. 153.). This figure shows that the tunica granulosa (g^1) and retinacula (g^2) are discharged with the ovum (par. 150.). The ovum is in the "second stage" of development (par. 141.).

Fig. 97. Rabbit. The same ovum on a larger scale. It is in the "second stage" of development (par. 141.).

Fig. 98. Rabbit. The ovisac as obtained, free from its covering, a few hours after the expulsion of the ovum from the ovary. It presents the orifice ($\frac{1}{4}'''$ in length) through which the ovum escaped (par. 154.).

Fig. 99. Hog (*Sus Scrofa*, LINN.). An ovum in which are seen the effects of incipient absorption *post coitum* (par. 158 to 160.). The yelk (*d*) is becoming fluid, a change which takes place first around the germinal vesicle (par. 159.). Dark globules are seen on the surface of the ovum.

Fig. 100. Hog. An ovum in which absorption (*post coitum*) has proceeded farther than in the last figure. The ovum has become elliptical (par. 158 to 160.).

Fig. 101. Rabbit. An ovisac (found in the ovary *post coitum*) apparently about to be absorbed. The ovum, and the germinal vesicle (*c*) have become elliptical, and the yelk (*d*) is in the state of fluid (par. 158 to 160.).

Fig. 102. Hog. An ovisac found in the infundibulum, apparently in the course of being absorbed (par. 162 to 165. 297.).

PLATE VI.

Fig. 103. Rabbit. *α*. An ovum of ten hours, and measuring in diameter $\frac{1}{11}'''$, found in the Fallopian tube, at the distance of an inch from the infundibulum. It is in the "third stage" of development (par. 170.). *β*. The same ovum crushed, to show the strength of the proper membrane of the yelk (*e*), which is unbroken and still contains the yelk, though forced through the ruptured membrane *f*. (par. 171.).

Fig. 104. Rabbit. *α*. An ovum of forty-one hours, and measuring in diameter $\frac{1}{11}'''$, found about an inch from the infundibulum in the Fallopian tube. It is in the "fourth stage" of development. The chorion (*cho.*) is visible, and closely invests the membrane *f*. *β*. Another ovum of the same Rabbit found further advanced into the tube. It has been crushed, and shows the chorion (*cho.*) now separated from the membrane *f*. (par. 172.).

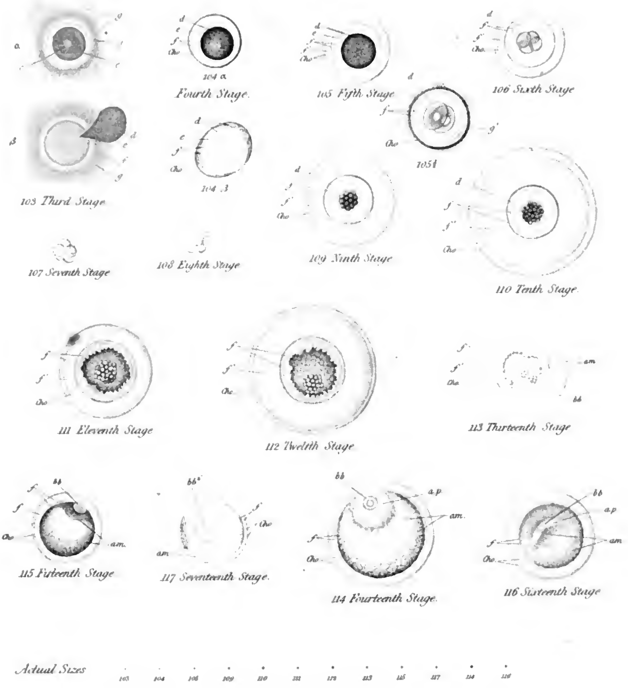
Fig. 105. Rabbit. An ovum of 35¾ hours, and measuring in diameter $\frac{1}{10}'''$, found in the Fallopian tube near its middle. It is in the "fifth stage" of development (par. 173.).

Fig. 105½. Rabbit. An ovum of twenty-four hours, and measuring in diameter $\frac{1}{11}'''$, found in the Fallopian tube at its middle part. It is in a state between that represented in fig. 105 and that in fig. 106. (par. 174. *Note*, 315. 316.)

Fig. 106. Rabbit. An ovum of 35¾ hours, and measuring in diameter $\frac{1}{9}'''$, found with the ovum represented in fig. 105. It is in the "sixth stage" of development (par. 174.).

Fig. 107. Rabbit. Vesicles in the centre of an ovum somewhat larger than that in the preceding figure, found in the Fallopian tube. These vesicles are smaller and more numerous than those in the last stage, and larger and less numerous than those in the stage following. They represent the "seventh stage" of development (par. 175. 317.).

Embryology.



104 α. Fourth Stage.

105 Fifth Stage.

106 Sixth Stage.

103 Third Stage.

104 β

105 β

107 Seventh Stage.

108 Eighth Stage.

109 Ninth Stage.

110 Tenth Stage.

111 Eleventh Stage.

112 Twelfth Stage.

113 Thirteenth Stage.

115 Fifteenth Stage.

117 Seventeenth Stage.

114 Fourteenth Stage.

116 Sixteenth Stage.

(All the Figures represent Ova of the Rabbit,— in Fig^s 103 to 113, magnified 75 diameters, in Fig^s 114 to 117,— 50 diam.)

bb. The true Germ
bb'. Future Vascular Lamina of the Umbilical Vesicle
c. Germinal Vesicle
d. Yelk
e. Membrane originally investing the Yelk

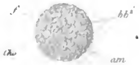
f. Thick transparent membrane of the ovarian Ovum,—"Zona pellucida"
f'. Fluid imbibed by the Chorion
g'. Tunica granulosa
Cho. Chorion, subsequently villous
am. Amnion
a.p. Area pellucida

ad Nat. del. Martin Barry

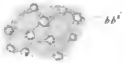
J. Basire sc.

Embryology

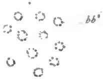
118
Eighteenth Stage.



119
Nineteenth Stage.

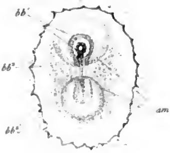


120
Twentieth Stage.



121
Twenty first Stage.

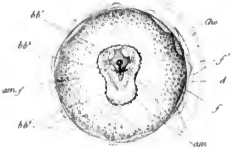
(40 Diam.)



122

121 D

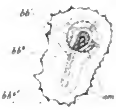
(50 Diameters)



121 A (⅛''')

121 C

(75 Diam.)



121 B

(40 Diam.)

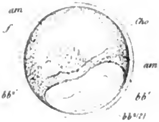
123

(120 Diam.)

125

126

(40 Diam.)



124 (abnormal ⅛''')

(8 Diam.)

127

Actual Sizes

(All the Figures are from Ova of the Rabbit.)

bb' Central Portion of the Germ
bb² Peripheral Portion of the Germ
bb³ Lamina subsequently vascular
d Escaped Yolk
f Thick transparent membrane, "Zona pellucida"
f' Fluid imbibed by the Chorion
Cho Chorion, subsequently villous
am Amnion
am.f Union of the membranes am and f

Ad Nat. del. Martin Barry

J. Basire

Fig. 108. Rabbit. Vesicles in the centre of an ovum from the Fallopian tube, representing the "eighth stage" of development (par. 176. 317.).

Fig. 109. Rabbit. An ovum of sixty-three hours, and measuring in diameter $\frac{1}{7}'''$, found in the Fallopian tube, within about an inch of the uterus. It is in the "ninth stage" of development (par. 178.).

Fig. 110. Rabbit. An ovum of 68¾ hours, and measuring in diameter $\frac{1}{5}'''$, found in the Fallopian tube within ¾ inch of the uterus. It is in the "tenth stage" of development (par. 180.).

Fig. 111. Rabbit. An ovum of 79½ hours, and measuring in diameter $\frac{1}{7}'''$, found in the uterus near the Fallopian tube. It is in the "eleventh stage" of development (par. 184.).

Fig. 112. Rabbit. An ovum of eighty-six hours, and measuring in diameter $\frac{1}{7}'''+$, found in the uterus within an inch of the Fallopian tube. It is in the "twelfth stage" of development (par. 185.).

Fig. 113. Rabbit. An ovum of 102½ hours, and measuring in diameter $\frac{1}{7}'''$, found in the uterus near its middle. It is in the "thirteenth stage" of development (par. 186. 187.).

Fig. 114. Rabbit. An ovum of 105 hours, and measuring in diameter $\frac{1}{4}'''$, found in the uterus ¾ inch from the Fallopian tube. It is in the "fourteenth stage" of development (par. 189.).

Fig. 115. Rabbit. An ovum of 94½ hours, and measuring in diameter $\frac{1}{6}'''$, found in the uterus near the Fallopian tube. It is in the "fifteenth stage" of development (par. 190. 191.).

Fig. 116. Rabbit. An ovum of 97¾ hours, and measuring in diameter $\frac{1}{5}'''$, found in the uterus about an inch from the Fallopian tube. It is in the "sixteenth stage" of development (par. 192.).

Fig. 117. Rabbit. An ovum of 97¾ hours, and measuring in diameter $\frac{1}{6}'''$, found in the uterus. It is in the "seventeenth stage" of development (par. 193 to 196.).

PLATE VII.

Fig. 118. Rabbit. An ovum of ninety-five hours, and measuring in diameter $\frac{1}{4}'''$, found in the uterus an inch from the Fallopian tube. It is in the "eighteenth stage" of development (par. 197 to 200.).

Fig. 119. Rabbit. An ovum of 108½ hours, and measuring in diameter $\frac{1}{4}'''+$, found in the uterus near the Fallopian tube. It is in the "nineteenth stage" of development (par. 201.).

Fig. 120. Rabbit. A portion of the future umbilical vesicle in an ovum of 107½ hours, and measuring $\frac{1}{5}'''$, found in the uterus 1¾ inch from the Fallopian tube. It represents the "twentieth stage" of development (par. 202.).

Fig. 121. Rabbit. A portion of the future umbilical vesicle in an ovum of $111\frac{1}{2}$ hours, and measuring in diameter $\frac{1}{5}'''$, found in the uterus an inch from the Fallopian tube. It represents the "twenty-first stage" of development (par. 203.).

Fig. 121 A. Rabbit. An ovum of $111\frac{1}{2}$ hours, and measuring in diameter $\frac{1}{5}'''$, found in the uterus an inch from the Fallopian tube (par. 205. 206.). Drawn as seen lying in kreosote water (par. 239.). The embryo is described in pars. 209. and 212 to 215.

Fig. 121 B. Rabbit. The embryo (bb^1 and bb^2.) and incipient amnion (*am.*). The embryo is in a stage succeeding that in fig. 121 A (par. 212 to 215.).

Fig. 121 C. Rabbit. The most central portion of the embryo in fig. 121 B, on a larger scale (pars. 213. 214.).

Fig. 121 D. Rabbit. The central portion (bb^1) of the germ, from an ovum of 103 hours, and measuring in diameter $\frac{1}{5}'''$. *am. f.* Union of the membranes *am.* and *f.*

Fig. 122. Rabbit. The embryo ($bb^1 + bb^2$) and incipient amnion (*am.*) from an ovum of $124\frac{1}{4}$ hours, and measuring in diameter $1''' +$, found in the uterus (pars. 214 to 216.).

Fig. 123. Rabbit. The central portion of the germ ($\frac{1}{4}'''$ in length) (par. 215.) from an ovum of $164\frac{1}{2}$ hours, and measuring in diameter more than $2\frac{1}{4}'''$, found in the uterus. This figure presents an instance of size being in advance of development (pars. 167. 218.).

Fig. 124. Rabbit. An ovum of $115\frac{1}{2}$ hours, and measuring in diameter $\frac{1}{3}'''$, found in the uterus. The embryo (bb^1) measured in this instance only $\frac{1}{7}'''$ ($= \frac{1}{70}$ of an English inch), thus presenting an example of development being greatly in advance of size (pars. 167. 218.).

Fig. 125. Rabbit. The embryo from the last figure, more highly magnified. It exhibits a tendency to become curved at the cephalic end, and transverse wrinkles, thereby produced (par. 219.).

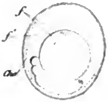
Fig. 126. Rabbit. The same, as seen after slight pressure had been applied (par. 219.).

Fig. 127. Rabbit. The visceral surface of the foundation of the central portion of the nervous system and vertebræ in an embryo of rather less than $1\frac{1}{2}'''$, from an ovum of $8\frac{1}{2}$ days, and measuring in diameter $6''' +$, found in the uterus (par. 217.).

PLATE VIII.

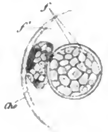
Fig. 128. Rabbit. The ovum of Plate VI. fig. 109, after being crushed. The dotted line denotes the inner surface of the subsequently villous chorion (*cho.*) (par. 178.).

Embryology.



128

129
Amnion



130

131
Fresh Water Mussel
(from Carus)

132

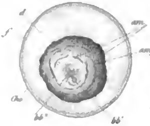
133
Aborted

134
Aborted

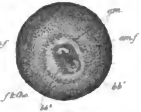
135
Aborted

136

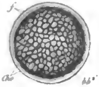
137
(Water)



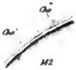
138
(Kreosote Water)



139
(Tar Water)

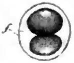


140
(Nitrate of Silver)



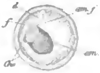
142

141

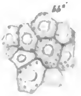


143

144



145



150

149

148

(All the Figures are from the Rabbit except Fig. 131)

bb' Central Portion of the Germ
bb^2 Peripheral Portion of the Germ
bb^3 Future vascular Lamina of the Umbilical Vesicle
d. Yolk. Escaped Yolk

f. Thick transparent membrane of the ovarian Ovum _ "Zona pellucida"
f' Fluid imbibed by the Chorion
Cho. Chorion subsequently villous. Villous Chorion
Cho'. Villi.

am. Amnion
am.f Union of the membranes am and f.

A Nat. del. Martin Barry

J. Basire sc.

Fig. 129. Rabbit. Vesicles of the future amnion, from an ovum of 94½ hours, and measuring in diameter $\frac{1}{8}'''$, found at the junction of the uterus and Fallopian tube (par. 190.).

Fig. 130. Rabbit. An ovum resembling that in Plate VI. fig. 110, after being crushed (par. 180.). The dotted line denotes the inner surface of the subsequently villous chorion (*cho.*) (par. 178.). Compare this ovum with fig. 131. from *Unio tumida* (par. 243.).

Fig. 131. From Carus. An ovum from the gills of *Unio tumida*. Compare with fig. 130. from the Rabbit (par. 243.).

Fig. 132. Rabbit. A portion of the network of which the subsequently vascular lamina of the umbilical vesicle consists in the "nineteenth stage" of development (Plate VI. fig. 119.) (par. 201.).

Fig. 133. Rabbit. An aborted ovum, found in the uterus (par. 226.).

Fig. 134. Rabbit. An aborted ovum, found in the uterus (par. 226.).

Fig. 135. Rabbit. An aborted ovum, found in the uterus (par. 226.).

Fig. 136. Rabbit. A vesicle, found in the Fallopian tube with others of the same kind (par. 227.).

Fig. 137. Rabbit. An ovum of 162¾ hours, and measuring in diameter $\frac{7}{10}'''$, found in the uterus. It is in a collapsed state from the effects of water (par. 231.).

Fig. 138. Rabbit. An ovum of $\frac{8}{10}'''$, drawn after lying three days in kreosote water (par. 239.).

Fig. 139. Rabbit. An ovum of $\frac{3}{4}'''$, drawn after lying four days in tar water (par. 241.).

Fig. 140. Rabbit. An ovum of $\frac{1}{4}'''$, showing the effects of nitrate of silver (par. 238.). The objects presenting in this ovum the appearance of a network, are vesicles such as those in Plate VIII. fig. 150.

Fig. 141. Rabbit. Villous tuft, measuring in diameter $\frac{1}{30}'''$, as seen on the chorion of an ovum from the uterus, measuring $\frac{7}{10}'''$ (par. 223.).

Fig. 142. Rabbit. Villous tufts seen in profile on the chorion (*cho.*) of an ovum from the uterus, measuring in diameter $\frac{7}{10}'''$ (par. 223.).

Fig. 143. Rabbit. An ovum of 23 hours, and measuring in diameter $\frac{1}{15}'''$, found in the Fallopian tube (par. 306.).

Fig. 144. Rabbit. An aborted ovum of 115½ hours, and measuring in diameter $\frac{1}{11}'''$, found in the uterus (par. 308. and *Note.* 317. *Note.*). It is in a state which, in reference to the central portion of the ovum, is between that represented in Plate VI. fig. 105. and that in fig. 105½.

Fig. 145. Rabbit. An ovum of 114¼ hours, and measuring in diameter $\frac{1}{2}'''$, found in the uterus. The membrane *am.* is adherent at a certain part to (*f*) the thick transparent membrane of the ovarian ovum (pars. 206. 208.). Drawn after lying six weeks in dilute spirit (par. 233.).

Fig. 148. Rabbit. The germ in its vesicle, as seen within the (imperfectly formed) mulberry-like structure while still in the centre of the ovum. The incipient separation of the germ into a central (*bb*[1]) and a peripheral (*bb*[2]) portion appeared to have been premature (par. 193. *Note*).

Fig. 149. Rabbit. Vesicles of which the incipient embryo was composed, in an ovum of 111½ hours, and measuring in diameter $\frac{1}{5}'''$, found in the uterus an inch from the Fallopian tube (par. 301. 305.). Drawn after lying 48 hours in kreosote water (par. 239.).

Fig. 150. Rabbit. Vesicles of the outer and subsequently vascular lamina of the umbilical vesicle, from an ovum of 107½ hours, and measuring in diameter $\frac{1}{5}''' +$, found in the uterus near the Fallopian tube (par. 211.). The interior of these vesicles was brought into view by nitrate of silver (par. 238.).

PLATE IX.

Fig. 151. Rabbit. A vesicle found under the mucous membrane at the junction of the uterus and Fallopian tube. The mulberry-like object in its centre was observed for half an hour to perform rotatory motions (pars. 282 to 287.).

Fig. 152. Rabbit. Vesicles from the surface of the mulberry-like object found in vesicles such as the one in fig. 151. (par. 285.)

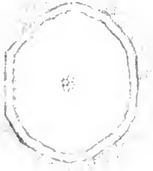
Fig. 153. Rabbit. An ovum of 17 hours, and measuring in diameter about $\frac{1}{10}'''$, found in the Fallopian tube near its middle (par. 173. *Note*. 221. to 225.).

Fig. 154. Rabbit. Vesicles found in a cream-white substance, filling to distention the left uterus, 157¾ *post coitum*. This substance—at first of about the same consistence as pus—coagulated very shortly after the uterus had been laid open. The vesicles varied in size from $\frac{1}{400}'''$ to $\frac{1}{300}'''$, most of them measuring about $\frac{1}{350}'''$.

Fig. 155. Rabbit. Two vesicles from the centre of an ovum of 26¼ hours, and measuring in diameter about $\frac{1}{10}'''$, found in the Fallopian tube on the ovarian side of its middle part. This figure represents a condition of the ovum between that in Plate VI. fig. 105½, and that in fig. 106 (par. 317.).

Embryology.

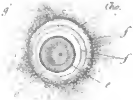
154.



151.

Vesicle;—the mulberry like object in the centre revolving on its own axis. (About 120 Diameters)

155.



153.

An Ovum of 17 hours from the middle of the Fallopian Tube, showing the mode of origin of the Chorion (Cho.). This membrane is rising from the thick transparent membrane or "Zona pellucida" (f'), surrounded by part of the Tunica granulosa (g'). (See also Pl. 7 fig. 103, representing an Ovum in which the Chorion (Cho.) was on all sides equidistant from the membrane, f.) (75 Diameters.)

152.

Vesicles from the surface of the mulberry like object in Fig. 151.

(The three Figures are from the Rabbit.)

Germinal Vesicle

e Yelk Ball

f Thick transparent membrane of the ovarian Ovum "Zona pellucida"

f'. Fluid imbibed by the Chorion

g' Tunica granulosa

Cho. Chorion, subsequently villous

M. Nat. del. Martin Barry. Basire sc.

XX. *Researches in Physical Geology. By* W. Hopkins, *Esq. M.A. F.R.S., Fellow of the Royal Astronomical Society, of the Geological Society, and of the Cambridge Philosophical Society.—First Series.*

Received November 22, 1838,—Read January 17, 1839.

On the Phenomena of Precession and Nutation, assuming the Fluidity of the Interior of the Earth.

§. *Preliminary Observations on the Refrigeration of the Globe.*

BEFORE I proceed to the discussion of the question which forms the principal subject of the present communication, I shall offer some general remarks on the refrigeration of the globe, as introductory not only to this memoir, but to others which I hope hereafter to bring under the notice of the Society.

In the first place, we may observe that there are two distinct processes of cooling, of which one belongs to bodies which are either solid or imperfectly fluid, and is termed cooling by *conduction*, and the other to masses in that state of more perfect fluidity which admits of a free motion of the component particles among themselves. In this case the cooling is said to take place by *circulation* or *convection*. The nature of the former process has been ascertained with considerable accuracy by experiment, and the laws of the phenomena have been made the subject of mathematical investigation, but of the exact laws of cooling by the latter process we are comparatively ignorant. It is manifest, however, that since *time* must be necessary for the transmission of the hotter and lighter particles from the central to the superficial parts of the mass, as well as for that of the colder and heavier particles in the opposite direction, the temperature must increase with the depth beneath the surface; and, moreover, that this increase will be the more rapid, the more nearly the fluidity of the mass approaches that limit at which this process of cooling would cease, and that by conduction begin, since the rapidity of circulation would constantly diminish as the fluidity should approximate to that limit. But still, even in this limiting case, it seems probable that the tendency to produce an equality of temperature throughout the mass will be much greater, and consequently the rate of increase of temperature in approaching the centre much less, than if the cooling of the mass had proceeded by conduction during the same time, the conductive power being very small.

If the matter composing the globe was originally in a high state of fluidity from heat, the process of cooling would undoubtedly, in the first instance, be by circulation. The manner in which the transition will take place from this mode of refrigeration to that by conduction, depends on certain conditions, of which, in our speculations on this subject, it is important to form a distinct conception.

Since the heat increases with the distance from the surface while the mass is cooling by circulation, the tendency to solidification, so far as it depends on this cause, will be greatest at the surface and least at the centre; but, on the other hand, the pressure is least at the surface and greatest at the centre; and consequently the tendency to solidify, as depending on this cause, will be greatest at the centre and least at the surface. To estimate this tendency under the joint influence of these causes, it would be necessary, in the first place, to know the law according to which the temperature increases in descending from the surface to the centre, while the mass is cooling by circulation; and secondly, the influence of the temperature in resisting solidification, as compared with that of the pressure in promoting it. These, however, are points on which we possess at present little or no experimental evidence, and therefore the only conclusion at which we can arrive is this,—that if the augmentation of the temperature with that of the depth be so rapid, that its effect in resisting the tendency to solidify be greater than that of the increase of pressure to promote it, there will be the greatest tendency to become *imperfectly fluid*, and afterwards to solidify in the superficial portions of the mass; whereas if the effect of the augmentation of pressure predominate over that of the temperature, this transition from perfect to imperfect fluidity, and subsequent solidity, will commence at the centre.

If we suppose the former of these cases to hold, it would appear that no incrustation of the surface could take place so long as any inferior portion of the mass retained its perfect fluidity, because as the superior particles should become condensed they would continually descend into the perfect fluid beneath, always supposing the mass in that state in which an increase of specific gravity would result from a decrease of temperature. The process of circulation would thus go on till every part of the mass should have lost that degree of more perfect fluidity, which admits of a motion of the particles among themselves being excited by their unequal refrigeration. The circulation, therefore, would cease nearly contemporaneously in every part of the mass, which would then begin to cool by conduction, rapidly at the surface exposed to the low temperature of the planetary space, and extremely slowly in the central parts, on account of the small conductive power of the matter composing the earth. Consequently the globe would consist, after a certain time, of an exterior solid crust, and interior fluid matter, of which the fluidity would increase in approaching the centre, where it might still approach to that more perfect fluidity which admits of cooling by convection. With reference, however, to the mechanical action of any forces producing either motion or hydrostatic pressure in the interior mass, the whole of it might, as an approximation, be considered perfectly fluid. No attempt has yet been made to determine the present probable thickness of the earth's crust, assuming it to have been originally in a state of fluidity, on account of the difficulty already mentioned, arising from our ignorance of the influence of high temperature in resisting solidification, compared with that of great pressure in promoting it. All that has hitherto been determined on the subject is, that the present state of the earth's surface may be consistent with the existence of a solid crust, of which the thickness is small compared with the earth's radius.

Let us now recur to the other case above mentioned, that in which the increase of pressure in descending towards the centre of the mass is supposed to have a greater effect in promoting solidification than the increase of temperature in preventing it. Supposing the mass to have been first in a state in which every part was cooling by convection, this process would first cease, and that of cooling by conduction begin at the centre, while the superior portion would still continue to cool by convection, so that these two processes would for a time be going on simultaneously in different parts of the mass. It is manifest, however, that the central portion, cooling by conduction, would constantly increase, while the exterior portion, cooling by convection, would constantly diminish, so that at length no part of the mass would be cooling by the latter process. Before it should reach this stage of the refrigeration the central portion of a mass so large as the earth might become perfectly solid, so that at the instant when the circulation should entirely cease, the whole might consist of a solid central nucleus, surrounded by the external portion still in a state of fusion, and of which the fluidity would vary continuously from the solidity of the nucleus to the fluidity of the surface, where, at the instant we are speaking of, it would be just such as not to admit of circulation.

When the mass should have arrived at this stage of the cooling, a change would take place in the process of solidification, which it is important to remark. The superficial parts of the mass must in all cases cool the most rapidly, and now (in consequence of the imperfect fluidity) being no longer able to descend, a *crust* will be formed on the surface, from which the process of solidification will proceed far more rapidly downwards, than upwards on the solid nucleus. Consequently, then, our globe would arrive at that state, according to the mode of cooling we are now considering, in which it would be composed of a solid shell, and a solid central nucleus, with matter in a state of fusion between them, the fluidity of which, however, would necessarily be less than that which might exist in the fused mass very near the centre in the case previously considered.

With respect to the thickness of the shell which may be consistent with the present appearances of the earth's surface, the same conclusion will hold as in the former case, i. e. it may be small compared with the earth's radius. What would be the radius of the solid nucleus at the instant of the first incrustation of the surface, or that which would correspond to any assigned thickness of the exterior shell, it is quite impossible to determine from the want of all experimental evidence respecting the tendency of great pressure to promote solidification at very high temperatures, and our ignorance of the temperature at which the superficial incrustation of a large mass would begin, when exposed to the temperature of the planetary space. It is, therefore, manifestly impossible to decide by any such reasoning as the above, whether the exterior shell and solid nucleus are now united, or are separated by matter still in a state of fusion[*].

Upon the whole, reasoning such as the above can lead us to nothing more definite than the following conclusions respecting the actual state of the earth, assuming it to have once been in a state of perfect fluidity.

(1.) It may consist of a solid exterior shell and an internal mass in a state of fusion, of which the fluidity is greatest at the centre; and it is possible that the thickness of the shell may be small compared with its radius, and the fluidity at the centre may approximate to that which would admit of cooling by convection.

(2.) It may consist of an exterior shell, and a central solid nucleus, with matter in a state of fusion between them. The thickness of the shell, as well as the radius of the solid nucleus, may possibly be small compared with the radius of the earth. The fluidity of the intervening mass must necessarily be considerably more imperfect than that which would just admit of cooling by circulation.

(3.) The earth may be solid from the surface to the centre.

It appears then that the direct investigation of the manner of the earth's refrigeration, assuming its original fluidity from heat, still leaves us in a state of perfect uncertainty as to the actual condition of its central parts, not from any imperfection in the mathematical part of the investigation, but from the want of the experimental determination of values which it must ever be found extremely difficult, if not impossible, to obtain with accuracy. Under these circumstances, we are naturally led to consider whether any other more indirect test may be found of the truth of the hypothesis of central fluidity. In reflecting on this subject, it occurred to me some time ago, that such a test might possibly be found in the delicate but well-defined phenomena of precession and nutation. The connexion between these phenomena and the interior fluidity will at once be seen by those accustomed to physical investigations of this nature; since it is manifest, that the direct action of those forces which produce the precessional motion of the earth's pole must be entirely different on the interior part of the earth, if that part be fluid, to that which must be exerted, if the interior part be solid. It becomes, therefore, a matter of interest to examine how far the internal fluidity may consist with the observed phenomena of the precessional motion of the pole. These phenomena have been shown to be perfectly in accordance with the internal solidity of the earth under certain hypotheses, which may be deemed perfectly reasonable, respecting the law of density; but so far from any attempts having been hitherto made to determine what would be the precessional motion on the supposition of interior fluidity, I am not aware that the problem has been before suggested. I shall now proceed to its solution, which forms the principal object of the present memoir.

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[*] M. Poisson, was, I believe, the first to advocate the hypothesis of the solidification of the earth having commenced from the centre, and has stated in general terms that, in such case, it would proceed to the surface which would be the last to solidify (Théorie de la Chaleur, p. 428.). It is manifest, however, from what has been advanced, that this could not be literally correct, but that the solidification must necessarily commence at the surface before the whole internal portion had become solid. The distinction is of little consequence as respects the object which M. Poisson had in view, but is of the highest importance with reference to Geological speculation, because it shows, that, supposing the earth once to have been fluid, it must be now or have been at some antecedent epoch in that state in which a solid exterior crust rests on an imperfectly fluid and incandescent mass beneath. It forms no part of my immediate object, to consider whether the hypothesis of this being or having been once the state of our planet, best enables us to account for the igneous matter which has been injected so generally into the sedimentary portion of the earth's crust, but it is important to know, that this state of the earth, assuming its original fluidity, is one through which it must necessarily have passed in the course of its refrigeration, whatever might be the process of its solidification.

On Precession and Nutation; assuming the Fluidity of the Interior of the Earth.

In the present memoir I shall investigate the amount of the luni-solar precession and nutation, assuming the earth to consist of a solid spheroidal shell filled with fluid. To present the problem under its most simple form I shall first suppose the solid shell to be bounded by a determinate inner spheroidal surface, of which the ellipticity is equal to that of the outer surface, the change from the solidity of the shell to the fluidity of the included mass not being gradual but abrupt. I shall also here suppose both the shell and fluid homogeneous and of equal density. From this I propose in a future memoir to pass to the case in which the earth is considered as heterogeneous.

§. *Statement of the Problem.*

1. If S denote the position of the sun, A the centre of the earth, A P its axis of instantaneous rotation, the sun's attraction tends to produce an angular velocity of the earth about an axis through A, and perpendicular to the plane S A P. The moving force producing this rotation (supposing the earth a homogeneous spheroid),

$$= \frac{3\mu}{2 r_{,}^3} \cdot \frac{4\pi}{15} a_{,}^2 c_{,} (a_{,}^2 - c_{,}^2) \sin 2\Delta^{*},$$

where

μ = absolute force of the sun's attraction.

Δ = sun's polar distance.

$r_{,} = \mathrm{S\,A}$.

$a_{,}$ = equatorial radius.

$c_{,}$ = polar radius.

Also the moment of inertia of the spheroid about the axis of this rotatory motion,

$$= \frac{4\pi}{15} k\, a_{,}^2 c_{,} (a_{,}^2 + c_{,}^2).$$

Consequently the accelerating force of rotation

$$= \frac{3}{2} \cdot \frac{\mu}{r_{,}^3} \cdot \frac{a_{,}^2 - c_{,}^2}{a_{,}^2 + c_{,}^2} \sin 2\Delta$$

$$= \frac{3}{2} \cdot \frac{\mu}{r_{,}^3}\, \epsilon \sin 2\Delta$$

(ϵ = ellipticity of the spheroid); and if we denote this quantity by α, and the diurnal

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[*] AIRY's Tracts, Precession and Nutation.

angular velocity of the earth by ω, the angular velocity of A P about A will $= \frac{\alpha}{\omega}$, the instantaneous motion of P being perpendicular to the plane S A P[*].

2. But let us now suppose the spheroid hollow, the hollow part being spherical, and having its centre coincident with that of the spheroid. The moving force of rotation will be unaltered, but the moment of inertia will

$$= \frac{4\pi}{15} k\, a_{,}^2 c_{,} (a_{,}^2 + c_{,}^2) - \frac{8\pi}{15} k\, r^5$$

(r = radius of the hollow sphere). Therefore α will now

$$= \frac{3}{2} \cdot \frac{\mu}{r_{,}^3} \cdot \frac{a_{,}^2 c_{,} (a_{,}^2 + c_{,}^2)}{a_{,}^2 c_{,} (a_{,}^2 + c_{,}^2) - 2 r^5} \sin 2\Delta,$$

which, if r be considerable, will be much greater than its former value.

3. Again, let us suppose this hollow sphere filled with matter in a state of perfect fluidity. The pressures of this fluid on the interior spherical surface of the shell containing it being normal pressures (whatever be the causes producing them), their directions will all pass through the centre of the spheroid, and cannot therefore influence the rotatory motion we are now considering; and since there will be no friction with the assumed perfect fluidity of the interior matter, the value of α will be precisely the same as that above stated, when the internal sphere is entirely empty. A much greater motion of the pole would therefore result from this constitution of the spheroid than if it were perfectly solid; and it would, moreover, be entirely independent of the position of the axis of rotation of the internal fluid.

4. If the internal surface of the solid shell be spheroidal instead of spherical, the directions of the fluid normal pressures will no longer pass through an axis through the centre of the earth; and when the axes of diurnal rotation of the solid shell, and of the internal fluid do not coincide (as must generally be the case from the different actions of the sun and moon on the solid shell and on the fluid contained in it), the fluid pressure arising from the centrifugal force will introduce a new and important element into the calculation of the precessional motion of the pole. I shall now proceed to the determination of this motion on the hypotheses previously stated.

§. *Formation of the Differential Equation for the Motion of the Pole.*

5. Conceive a sphere of radius unity described about the centre of the earth, which centre we shall always denote by A. Let Π (fig. 1.) be the point in which a line through the centre and perpendicular to the plane of the ecliptic meets the sphere; and P and P′ the points in which it is met respectively by the axes of instantaneous rotation of the solid shell and of the internal fluid mass[†]. Let P and P′ be referred to the small circle O M, of which Π is the pole, and to great circles Π P M, Π P′ M′ respect-

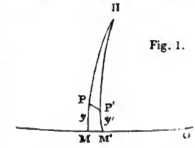

Fig. 1.

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[*] Airy's Tracts, p. 197.

[†] The axis of instantaneous rotation may be regarded as coincident with the spheroidal axis of the earth;

ively, Π M being very nearly equal to the obliquity of the ecliptic. Take O an arbitrary fixed point in the small circle, and let

$$\mathrm{O\,M} = x \qquad \mathrm{O\,M'} = x'$$
$$\mathrm{M\,P} = y \qquad \mathrm{M'\,P'} = y',$$

y, y' and $x - x'$ will be in general very small, and may, therefore, be considered as straight lines. Our object will be to form a system of four simultaneous differential equations, the integration of which will give x, y, x', and y' as functions of t. For this purpose I shall first consider the *arguments* of the different terms in the expressions for $\frac{dx}{dt}$, $\frac{dx'}{dt}$, $\frac{dy}{dt}$ and $\frac{dy'}{dt}$, which severally express the effects of the different physical causes affecting the motions of P and P', postponing the calculation of the numerical values of the *coefficients* till we shall have integrated our differential equations, as we shall then have the advantage of knowing what degree of accuracy may be essential in the determination of these values.

I. *The Attraction of the Sun on the Solid Shell.*—This will produce effects of precisely the same kind as if the spheroid were solid, but with different coefficients (Art. 2.), and therefore, if the motion of P depended on this cause alone, we should have

$$\frac{dx}{dt} = \mathrm{A}_1 - \mathrm{B}_1 \cos 2\,(n\,t + \lambda)$$
$$\frac{dy}{dt} = \qquad \mathrm{D}_1 \sin 2\,(n\,t + \lambda),$$

(where $n\,t + \lambda$ is the longitude of the sun at the time t), these being the forms of the expressions which give the precessional motion of the pole, and its motion of nutation as far as they depend on the sun's action.

II. *The Attraction of the Moon on the Shell.*—This alone would give us

$$\frac{dx}{dt} = \mathrm{A}_2 - \mathrm{B}_2 \cos 2\,(n'\,t + \lambda')$$
$$\frac{dy}{dt} = \qquad \mathrm{D}_2 \sin 2\,(n'\,t + \lambda'),$$

where $n' = \frac{\pi}{\text{period of ☽'s node}}$.

III. *The Interior Pressure on the Shell from the Attraction of the Sun on the Fluid Mass.*—If the whole mass of the earth were perfectly fluid, and its undisturbed form spherical, the attraction of the sun alone would transform this sphere into a prolate spheroid, of which the longer axis would lie in the line through S and A, the centres of the sun and earth; and similarly if the interior surface of the solid shell which we suppose to contain the internal fluid were spherical, the sun's action would tend to make this fluid assume the spheroidal form just mentioned, and would consequently produce a fluid pressure on the interior surface of the solid shell, which

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for the greatest angular separation will be of the same order as ϵ . P A P', and may therefore be neglected in comparison with P A P'.

would be equal at all points similarly situated with respect to the line just mentioned through the centres of the sun and earth. If the interior surface of the shell be spheroidal, but of small eccentricity, very nearly the same effect will be produced. The pressure in this case will be exactly equal at points similarly situated with respect to a plane through the sun and the axis of rotation (A P) of the shell, and will consequently tend to communicate a rotatory motion to the shell about an axis perpendicular to this plane and through the earth's centre; *i.e.* about the same axis as that about which the attraction mentioned in (I.) tends to communicate a rotatory motion. Also the effects of this pressure must *recur* with recurring positions of the sun exactly in the same manner as the effects of the sun's attraction just alluded to. Hence the terms depending on this cause will be of the same form as those in (I.), as will, in fact, be proved to be the case when we come to investigate their exact value. They will, therefore, give us

$$\frac{dx}{dt} = A_3 - B_3 \cos 2 (nt + \lambda)$$

$$\frac{dy}{dt} = \qquad D_3 \sin 2 (nt + \lambda).$$

IV. *The Interior Pressure on the Shell from the Attraction of the Moon on the Fluid Mass.*—This will give us terms similar to those arising from the sun's action. From this cause alone, therefore, we should have

$$\frac{dx}{dt} = A_4 - B_4 \cos 2 (n' t + \lambda')$$

$$\frac{dy}{dt} = \qquad D_4 \sin 2 (n' t + \lambda').$$

V. *The Interior Pressure on the Shell from the Centrifugal Force of the Fluid Mass.*—When P and P′ do not coincide, the interior fluid mass will tend, from the effect of centrifugal force, to assume a form different from that of the interior surface of the solid shell. Thus normal pressures will be produced on the interior surface of the shell; and they will manifestly act symmetrically with respect to a plane through P, P′ and A the centre of the earth, i. e. through the axes of rotation of the solid shell and of the fluid mass. Consequently the tendency of these pressures will be to communicate a motion of rotation to the shell about an axis through A, and perpendicular to this plane; and the consequent motion of P, if this force alone acted on the shell, would be perpendicular to P′ P, the axis of rotation of the shell having, from this cause, an angular velocity in space $= \frac{\alpha''}{\omega}$ (Art. 1.) α'', being the quantity analogous to α in the article referred to; or since P and P′ are supposed to be on the surface of a sphere whose radius is unity, $\frac{\alpha''}{\omega}$ will be the *linear* velocity of P perpendicular to P′ P. Now when we come to the calculation of the quantities involved in these investigations, we shall find that $\frac{\alpha''}{\omega} = \gamma_1 \sin 2\beta$, where γ_1 is a constant quantity depending on the diurnal angular velocity (ω), and on the magnitudes and ellipticity of the fluid spheroid and solid shell; and where $\beta =$ the angle P A P′, or $=$ the line P′ P. Consequently,

$$\frac{\alpha''}{\omega} = \gamma_1 \sin 2 \,.\, \mathrm{P'\,P},$$

or, since P′ P will always be extremely small, the linear velocity of P perpendicular to P′ P,

$$= \frac{\alpha''}{\omega} = 2\,\gamma_1 \,.\, \mathrm{P'\,P};$$

and resolving this in directions parallel and perpendicular to M′ M, we have (ψ being the angle which P′ P makes with the axis of x)

$$\frac{d\,x}{d\,t} = -\,2\,\gamma_1 \,.\, \mathrm{P'\,P} \,.\, \sin\psi = -\,2\,\gamma_1\,(y - y')$$

$$\frac{d\,y}{d\,t} = 2\,\gamma_1 \,.\, \mathrm{P'\,P}\cos\psi = 2\,\gamma_1\,(x - x').$$

6. If we now take the sum of the different terms which express the effects of the several causes affecting the motion of P, we obtain for the complete values of $\frac{d\,x}{d\,t}$ and $\frac{d\,y}{d\,t}$,

$$\frac{d\,x}{d\,t} = (\mathrm{A}_1 + \mathrm{A}_2 + \mathrm{A}_3 + \mathrm{A}_4) - (\mathrm{B}_1 + \mathrm{B}_3)\cos 2\,(n\,t + \lambda) - (\mathrm{B}_2 + \mathrm{B}_4)\cos 2\,(n'\,t + \lambda') \\ -\,2\,\gamma_1\,(y - y');$$

$$\frac{d\,y}{d\,t} = (\mathrm{D}_1 + \mathrm{D}_3)\sin 2\,(n\,t + \lambda) + (\mathrm{D}_2 + \mathrm{D}_4)\sin 2\,(n'\,t + \lambda') \\ +\,2\,\gamma_1\,(x - x');$$

or putting

$$\mathrm{A}_1 + \mathrm{A}_2 + \mathrm{A}_3 + \mathrm{A}_4 = \mathrm{A}$$

$$\mathrm{B}_1 + \mathrm{B}_3 = \mathrm{B}$$

$$\mathrm{B}_2 + \mathrm{B}_4 = \mathrm{B}'$$

$$\mathrm{D}_1 + \mathrm{D}_3 = \mathrm{D}$$

$$\mathrm{D}_2 + \mathrm{D}_4 = \mathrm{D}'$$

$$\left.\begin{aligned} \frac{d\,x}{d\,t} + 2\,\gamma_1\,(y - y') &= \mathrm{A} - \mathrm{B}\cos 2\,(n\,t + \lambda) - \mathrm{B}'\cos 2\,(n'\,t + \lambda') \\ \frac{d\,y}{d\,t} - 2\,\gamma_1\,(x - x') &= \mathrm{D}\sin 2\,(n\,t + \lambda) + \mathrm{D}'\sin 2\,(n'\,t + \lambda') \end{aligned}\right\} \quad . \quad . \quad (\mathrm{A}.)$$

§. *Motion of the Internal Fluid.*

7. When any accelerating forces, X, Y, Z, act upon a homogeneous fluid mass of which the whole surface or any part of it is free, we have two conditions of equilibrium, viz. that $\mathrm{X}\,d\,x + \mathrm{Y}\,d\,y + \mathrm{Z}\,d\,z$ must be a perfect derivation of a function of the three

independent variables x, y, z, and that $X\,dx + Y\,dy + Z\,dz = 0$, must be the differential equation to the free surface. If however no part of the surface of the fluid is free, as when the whole mass is contained in a rigid shell which it entirely fills, the former of these conditions is the only essential one of equilibrium. Also if there be several sets of forces which separately satisfy this condition when referred to different systems of coordinate axes, it will manifestly be satisfied by all these sets of forces taken conjointly; and if any proposed set of forces do not satisfy it, we may still omit, in the determination of the motion resulting from these forces, those terms in the expressions for X, Y, and Z, which taken conjointly do satisfy the analytical condition now spoken of. These considerations will materially simplify the following investigations.

8. We have now to consider the tendency of the forces acting on the internal fluid to put it in motion.

I. *Disturbing Force of the Sun.*—Let x, y, z be the coordinates of any particle (Q) of the internal fluid, the centre of the earth (A) being the origin, the line joining the centres of the earth and sun (A S) the axis of x, and the axis of z being perpendicular to the plane of the ecliptic. We shall then have

$$\text{the disturbing force on Q parallel to } x = 2\,\frac{\mu}{r_{,}^{3}}\cdot x$$

$$\text{the disturbing force on Q parallel to } y = -\,\frac{\mu}{r_{,}^{3}}\cdot y$$

$$\text{the disturbing force on Q parallel to } z = -\,\frac{\mu}{r_{,}^{3}}\cdot z,$$

substituting these quantities for X, Y, and Z respectively in the expression $X\,dx + Y\,dy + Z\,dz$, it manifestly becomes a perfect derivative. Consequently the condition of equilibrium is satisfied, and the action of the sun has no tendency to communicate motion to the internal fluid.

II. *Disturbing Force of the Moon.*—The investigation and result are precisely the same as for the sun.

III. *Centrifugal Force.*—In investigating the equations of motion for the solid shell, it has been assumed (Arts. 4. 5.) that the spheroidal axis of the shell will not generally be coincident with the axis A B′ of rotation, which is now proved to be true, since the disturbing forces of the sun and moon, while they produce a motion of the shell, cause no motion by their immediate action in the fluid. Let B′ A b' (fig. 2.) be the axis of rotation of the interior fluid, and suppose the spheroidal axis first to coincide with it, the dotted ellipse then representing the section of the interior surface of the solid shell. The shell, its form being supposed coincident with that of equilibrium of the fluid, will, in this case, produce no constraint on the fluid motion; but conceive the shell to be afterwards brought into the position represented by the continuous ellipse, A B being its spheroidal axis, while B′ b' shall still represent the instantaneous axis of rotation of the fluid. It is manifest that the planes of rotatory motion of the fluid particles near B′ and b' can no longer, as in the former case, be perpendicular to A B′, but must be constrained to move in planes very nearly parallel to the tangent planes at B′ and b'; and it is also sufficiently obvious that whatever effect is produced on the planes of motion of the above particles, a similar effect must be produced on those more remote from B′ and b'. Moreover, the mutual action of contiguous particles situated in contiguous planes of rotation will necessarily preserve a very approximate parallelism of these planes throughout the mass. We may conclude, therefore, that the instantaneous planes of rotation will always approximate, in a greater or less degree, to parallelism with the tangent planes at B′ and b', the extremities of the instantaneous axis of rotation of the fluid. In the investigations immediately following, we shall assume this to be accurately true, and shall prove subsequently the accuracy of the approximation to the true motions thus obtained. If M N′ be one of these planes of motion, M N perpendicular to A B′, and ι = angle N M N′, we shall have $\iota = 2\,\epsilon\,\beta$, as may be easily proved.

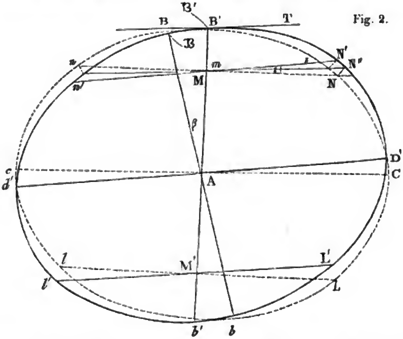

Fig. 2.

9. The sections of the interior surface of the shell made by these planes of rotatory motion will be similar ellipses, so that the angular velocity of rotation will no longer be accurately uniform. If, however, e' be the eccentricity of these sections, ϵ the ellipticity of the spheroid, and β the angle B A B′ (which will always be extremely small), it is easily shown that

$$e'^2 = 2\,\epsilon\,\beta.$$

This is so small that we shall still consider the angular velocity uniform, which will be proved in the sequel to be correct to the degree of approximation to which it is requisite to carry our investigations.

We may proceed to determine the centrifugal force on the fluid.

10. Let A B′ be now taken for the common axis of z and z', A C, perpendicular to A B′, for that of x, and A D′, conjugate to A B′, for that of x', the axis of y being perpendicular to the plane B A B′, that of the paper. x, y, z will be the coordinates of any fluid particle (Q) referred to the rectangular system of coordinates, and x', y, z' those of the same particle referred to the system in which the axis of z' is oblique to the plane of $x' y$. Also D′ A C = N M N′ = ι (Art. 8. III.). Then if r' be the distance of Q from the axis of rotation of the fluid, measured in the plane of its motion, the whole centrifugal force on Q in the direction of $r' = \omega^2 r'$, which (since x' and y are rectangular) is equivalent to $\omega^2 x'$ parallel to the axis of x', and $\omega^2 y'$ parallel to that of y. Hence

$$X = \omega^2 x' \cos \iota = \omega^2 x,$$
$$Y = \omega^2 y,$$
$$Z = \omega^2 x' \sin \iota = 2\, \omega^2 \epsilon \beta \, . \, x \text{ (Art. 8.).}$$

These forces do not satisfy the conditions of equilibrium, and therefore the assumed position will not be one of equilibrium. The conditions would be satisfied, however, if the only forces were $\omega^2 x$ and $\omega^2 y$, and consequently the only force which would tend to produce motion would be Z, or $2\, \omega^2 \epsilon \beta \, . \, x$. This is therefore the only part of the centrifugal force of which it is here necessary to take account.

11. In determining the motion produced by this force Z, we may observe, that since it acts symmetrically with respect to the plane of $x z$, by which the interior surface of the shell is divided symmetrically, there can be no motion in directions perpendicular to that plane. The motion of each fluid particle must therefore be in a plane perpendicular to the axis of y, and must moreover be independent of y, since Z is so. Hence the determination of the motion is reduced to the case of fluid motion in one plane, where (the plane itself being taken for that of $x z$) each particle is acted on by the force $Z = 2\, \omega^2 \epsilon \beta \, . \, x$, and the boundary of the fluid is an ellipse whose ellipticity is ϵ, and whose centre is the origin of coordinates.

12. The general equation

$$d p = X\, d x + Z\, d z,$$

where X and Z are forces which maintain the fluid in equilibrium, is easily reduced to

$$d p = R\, d r + \Theta\, r\, d \theta,$$

where r and θ are polar coordinates of any fluid particle, R the accelerating force upon it in the direction of r, and Θ that in a direction perpendicular to the former. Hence we have the condition of equilibrium

$$\frac{d R}{d \theta} = \frac{d \, . \, \Theta r}{d r};$$

or if Θ be the force acting on the fluid, but $\Theta + \Theta'$ that which would produce equilibrium with R, we have

$$\frac{d R}{d \theta} = \frac{d \, . \, (\Theta r + \Theta' r)}{d r}.$$

Now in the case to which this condition is to be applied, we have (θ being measured from the axis of x, and r from the origin of x and z)

$$\begin{aligned} \mathrm{R} &= \mathrm{Z} \sin \theta \\ &= 2\, \omega^2\, s\, \beta\, r \cos \theta \sin \theta \\ &= \frac{k}{2}\, r \sin 2\, \theta, \qquad\qquad (k = 2\, \omega^2\, s\, \beta)\,; \end{aligned}$$

$$\begin{aligned} \Theta\, r &= \mathrm{Z} \cos \theta \,.\, r \\ &= k\, r^2 \cos^2 \theta, \\ \therefore \frac{d\,\mathrm{R}}{d\,\theta} &= k\, r \cos 2\, \theta, \\ \frac{d\,.\,\Theta\, r}{d\,r} &= 2\, k\, r \cos^2 \theta \\ &= k\, r\, (1 + \cos 2\, \theta). \end{aligned}$$

Substituting these values in the above equation,

$$k\, r \cos 2\, \theta = k\, r\, (1 + \cos 2\, \theta) + \frac{d\,.\,\Theta'\, r}{d\,r},$$

$$\therefore \frac{d\,.\,\Theta'\, r}{d\,r} = -\, k\, r,$$

$$\Theta'\, r = -\, \frac{k}{2}\, r^2 + \Phi\, (\theta)\,;$$

and since $\Theta'\, r$ must vanish with r, $\Phi\, (\theta)$ must $= 0$, and

$$\Theta' = -\, \frac{k}{2}\, r\,;$$

or if forces $\Theta' = -\, \frac{k}{2}\, r$, and $\mathrm{Z} = k\, x$ act on each fluid particle, there will be no motion.

Now suppose forces $\frac{k}{2}\, r$ and $-\, \frac{k}{2}\, r$ equal and in opposite directions to act on each particle perpendicular to r, together with Z. The motion produced by Z will not be affected by this superposition. But the forces Z ($= k\, x$) and $-\, \frac{k}{2}\, r$ are in equilibrium, and therefore the motion produced by Z must be the same as that which would be produced by $\frac{k}{2}\, r$, acting perpendicular to r.

13. Since the motion we are considering is in space of two dimensions, the surface of the fluid must be defined by some plane curve, if the particular form of which the result at which we have just arrived is quite independent, being subject only to the condition that no part of the fluid surface shall be free. Let us suppose the curve to be a circle, of which the centre is the origin of coordinates. The angular accelerating force on each particle $= \frac{k}{2}$, and is, therefore, the same for every particle. Also the

reaction of the surface would, in this case, have no effect on the angular motion of the fluid. Consequently the angular velocity generated by Z in a unit of time would $= \frac{k}{2}$.

14. If the boundary of the fluid be an ellipse of which the centre is the origin and the eccentricity very small, the same result will manifestly be very approximately true.

This last is the case, in which it was necessary to determine the angular motion (Art. 11.). It follows that the angular velocity of the internal fluid mass round the axis of y, which would be generated by the force Z in a unit of time $= \frac{k}{2}$, or (substituting the proper value of k) it $= \omega^2 \epsilon \beta$, neglecting quantities of the order $\epsilon^2 \beta$.

15. This angular velocity will be compounded with that about the axis of z (ω). Now if we again suppose the fluid mass to be spherical, it would manifestly move precisely as if it were solid, since the angular velocities ω and $\omega^2 \epsilon \beta$ about the axes of z and y respectively are common to all the particles of the mass, and the axis of instantaneous rotation would consequently have an angular motion in space perpendicular to the plane B A B′ (fig. 2.) and $= \frac{\alpha'}{\omega} = \omega \epsilon \beta$. If the fluid mass be spheroidal, as in our actual case, the ellipticity being small the same result will be very approximately true.

We may now proceed to the formation of the differential equations for the motion of the instantaneous axis of rotation of the interior fluid, or of the point P′ (fig. 1.).

§. *Formation of the Differential Equations for the Motion of* P′ (fig. 1.).

16. Since the angular velocity of A B′ (fig. 2.) is $\omega \epsilon . \beta$ (Art. 15.) in a direction perpendicular to the plane B A B′, the linear velocity of P′ (fig. 1.) will also be $\omega \epsilon . \beta$, or $\omega \epsilon . \mathrm{P\,P'}$; or if $\omega \epsilon = 2 \gamma_2$, the linear velocity of P′ perpendicular to $\mathrm{P\,P'} = 2 \gamma_2 . \mathrm{P\,P'}$. This is exactly similar to the expression for the motion of P perpendicular to P P′ (Art. 5. V.), but it will be observed that the angular motion of the fluid about the axis perpendicular to the plane B A B′ (fig. 2.) which the centrifugal force tends to produce, is in the direction opposite to that of the angular motion of the shell which the fluid pressure on its interior surface, arising from this centrifugal force, tends to produce (Art. 14.). Hence to obtain the differential equations for the motion of P′ we have only to put $- \gamma_2$ for γ_1 in the equations of Art. 5. V. We thus have (now denoting by x y x' and y' the same quantities as in Art. 5.).

$$\left.\begin{aligned} \frac{dx'}{dt} - 2\gamma_2 (y - y') = 0 \\ \frac{dy'}{dt} + 2\gamma_2 (x - x') = 0 \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots \quad (\mathrm{B.})$$

§. *Integration of the Differential Equations for the Motions of* P *and* P'.

17. The equations (A) and (B) (Arts. 6, 16.) form a system of four simultaneous differential equations, viz.

$$\frac{dx}{dt} + 2\gamma_1(y - y') = \mathrm{A} - \mathrm{B}\cos 2(nt + \lambda) - \mathrm{B}'\cos 2(n't + \lambda'), \quad . \quad . \quad . \quad (1.)$$

$$\frac{dy}{dt} - 2\gamma_1(x - x') = \qquad \mathrm{D}\sin 2(nt + \lambda) + \mathrm{D}'\sin 2(n't + \lambda'), \quad . \quad . \quad . \quad (2.)$$

$$\frac{dx'}{dt} - 2\gamma_2(y - y') = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

$$\frac{dy'}{dt} + 2\gamma_2(x - x') = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

(1.) × γ_2 + (3.) × γ_1 gives

$$\gamma_2\frac{dx}{dt} + \gamma_1\frac{dx'}{dt} = \gamma_2\mathrm{A} - \gamma_2\mathrm{B}\cos 2(nt + \lambda) - \gamma_2\mathrm{B}'\cos 2(n't + \lambda'),$$

and (2.) × γ_2 + (4.) × γ_1 gives

$$\gamma_2\frac{dy}{dt} + \gamma_1\frac{dy'}{dt} = \gamma_2\mathrm{D}\sin 2(nt + \lambda) + \gamma_2\mathrm{D}'\sin 2(n't + \lambda').$$

Integrating these two last equations,

$$\gamma_2 x + \gamma_1 x' = \gamma_2\mathrm{A}t - \frac{\gamma_2\mathrm{B}}{2n}\sin 2(nt + \lambda) - \frac{\gamma_2\mathrm{B}'}{2n'}\sin 2(n't + \lambda') + c_1, \quad . \quad . \quad (5.)$$

$$\gamma_2 y + \gamma_1 y' = -\frac{\gamma_2\mathrm{D}}{2n}\cos 2(nt + \lambda) - \frac{\gamma_2\mathrm{D}}{2n'}\cos 2(n't + \lambda') + c_2. \quad . \quad . \quad . \quad (6.)$$

To determine the arbitrary constants c_1 and c_2, let x, y, x' and y' each $= 0$ when $t = 0$. Then

$$c_1 = \frac{\gamma_2\mathrm{B}}{2n}\sin 2\lambda + \frac{\gamma_2\mathrm{B}'}{2n'}\sin 2\lambda',$$

$$c_2 = \frac{\gamma_2\mathrm{D}}{2n}\cos 2\lambda + \frac{\gamma_2\mathrm{D}'}{2n'}\cos 2\lambda'.$$

Equations (5.) and (6.) are two integrals of our four differential equations.

18. Substituting in (1.) and (2.) for y' and x', we obtain

$$\frac{dx}{dt} + 2(\gamma_1 + \gamma_2)y = 2c_2 + \mathrm{A} - \left(\mathrm{B} + \frac{\gamma_2\mathrm{D}}{n}\right)\cos 2(nt + \lambda)$$
$$- \left(\mathrm{B}' + \frac{\gamma_2\mathrm{D}'}{n'}\right)\cos 2(n't + \lambda'),$$

$$\frac{dy}{dt} - 2(\gamma_1 + \gamma_2)x = -2c_1 + \left(\mathrm{D} + \frac{\gamma_2\mathrm{B}}{n}\right)\sin 2(nt + \lambda)$$
$$+ \left(\mathrm{D}' + \frac{\gamma_2\mathrm{B}'}{n'}\right)\sin 2(n't + \lambda') - 2\gamma_2\mathrm{A}t.$$

Let $\gamma = \gamma_1 + \gamma_2, \qquad \mathrm{K} = \gamma_2\mathrm{A},$

$\mathrm{L} = 2c_2 + \mathrm{A}, \qquad \mathrm{L}' = -2c_1,$

$$M = B + \frac{\gamma_2 D}{n}, \qquad M' = B' + \frac{\gamma_2 D'}{n'},$$

$$N = D + \frac{\gamma_2 B}{n}, \qquad N' = D' + \frac{\gamma_2 B'}{n'}.$$

Then

$$\frac{dx}{dt} + 2\gamma y = L - M\cos 2(nt + \lambda) - M'\cos 2(n't + \lambda'),$$

$$\frac{dy}{dt} - 2\gamma x = L' + N\sin 2(nt + \lambda) + N'\sin 2(n't + \lambda') - 2Kt.$$

The integration of these equations will be easily effected by the method of the variation of the arbitrary constants. The simultaneous equations

$$\frac{dx}{dt} + 2\gamma y = 0,$$

$$\frac{dy}{dt} - 2\gamma x = 0$$

are satisfied by

$$\left.\begin{array}{l} x = C_1\cos 2\gamma t - C_2\sin 2\gamma t \\ y = C_1\sin 2\gamma t + C_2\cos 2\gamma t \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (C.)$$

and if we write equations (B.) under the form

$$\frac{dx}{dt} + 2\gamma y = \Phi(t),$$

$$\frac{dx}{dt} - 2\gamma x = \Psi(t),$$

we shall have, considering C_1 and C_2 as functions of t,

$$\frac{dC_1}{dt}\cos 2\gamma t - \frac{dC_2}{dt}\sin 2\gamma t = \Phi(t),$$

$$\frac{dC_1}{dt}\sin 2\gamma t + \frac{dC_2}{dt}\cos 2\gamma t = \Psi(t);$$

$$\therefore \frac{dC_1}{dt} = \cos 2\gamma t \,.\, \Phi(t) + \sin 2\gamma t \,.\, \Psi(t),$$

$$\frac{dC_2}{dt} = -\sin 2\gamma t \,.\, \Phi(t) + \cos 2\gamma t\, \Psi(t).$$

Each term in C_1 and C_2 corresponding to the several terms in $\Phi(t)$ and $\Psi(t)$ may be determined separately.

Let $\Phi(t) = L$, $\Psi(t) = L'$; then

$$\frac{dC_1}{dt} = L \,.\, \cos 2\gamma t + L'\sin 2\gamma t,$$

$$\frac{dC_2}{dt} = -L\sin 2\gamma t + L'\cos 2\gamma t;$$

$$\therefore C_1 = \frac{L}{2\gamma} \sin 2\gamma t - \frac{L'}{2\gamma} \cos 2\gamma t,$$

$$C_2 = \frac{L}{2\gamma} \cos 2\gamma t + \frac{L'}{2\gamma} \sin 2\gamma t.$$

Let $\Phi(t) = -M \cos 2(nt + \lambda)$,

$\Psi(t) = \quad N \sin 2(nt + \lambda)$; then

$$\frac{dC_1}{dt} = -M \cos 2\gamma t \,.\, \cos 2(nt + \lambda) + N \sin 2\gamma t \,.\, \sin 2(nt + \lambda),$$

$$\frac{dC_2}{dt} = \quad M \sin 2\gamma t \,.\, \cos 2(nt + \lambda) + N \cos 2\gamma t \,.\, \sin 2(nt + \lambda);$$

$$\therefore \frac{dC_1}{dt} = -\frac{M}{2}\left\{\cos 2(\overline{\gamma - n}\,t - \lambda) + \cos 2(\overline{\gamma + n}\,t + \lambda)\right\}$$

$$+ \frac{N}{2}\left\{\cos 2(\overline{\gamma - n}\,t - \lambda) - \cos 2(\overline{\gamma + n}\,t + \lambda)\right\};$$

and $\therefore C_1 = -\frac{M - N}{2} \cdot \frac{\sin 2(\overline{\gamma - n}\,t - \lambda)}{2(\gamma - n)} - \frac{M + N}{2} \frac{\sin 2(\overline{\gamma + n}\,t + \lambda)}{2(\gamma + n)}$:

$$\frac{dC_2}{dt} = \frac{M}{2}\left\{\sin 2(\overline{\gamma + n}\,t + \lambda) + \sin 2(\overline{\gamma - n}\,t - \lambda)\right\}$$

$$+ \frac{N}{2}\left\{\sin 2(\overline{\gamma + n}\,t + \lambda) - \sin 2(\overline{\gamma - n}\,t - \lambda)\right\};$$

and $\therefore C_2 = -\frac{M + N}{2} \cdot \frac{\cos 2(\overline{\gamma + n}\,t + \lambda)}{2(\gamma + n)} - \frac{M - N}{2} \cdot \frac{\cos 2(\overline{\gamma - n}\,t - \lambda)}{2(\gamma - n)}$.

Taking $\varphi(t) = -M' \cos 2(n't + \lambda')$,

$\psi(t) = \quad N' \sin 2(n't + \lambda')$,

we shall obtain in a similar manner

$$C_1 = -\frac{M' - N'}{2} \cdot \frac{\sin 2(\overline{\gamma - n'}\,t - \lambda')}{2(\gamma - n')} - \frac{M' + N'}{2} \cdot \frac{\sin 2(\overline{\gamma + n'}\,t + \lambda')}{2(\gamma + n')},$$

$$C_2 = -\frac{M' + N'}{2} \cdot \frac{\cos 2(\overline{\gamma + n'}\,t + \lambda')}{2(\gamma + n')} - \frac{M' - N'}{2} \cdot \frac{\cos 2(\overline{\gamma - n'}\,t - \lambda')}{2(\gamma - n')}.$$

Let $\varphi(t) = 0$, $\psi(t) = -2Kt$; then

$$\frac{dC_1}{dt} = -2Kt \,.\, \sin 2\gamma t,$$

$$\frac{dC_2}{dt} = -2Kt \,.\, \cos 2\gamma t;$$

$$\therefore C_1 = -2K\left(\frac{\sin 2\gamma t}{4\gamma^2} - \frac{\cos 2\gamma t}{2\gamma} \,.\, t\right),$$

$$C_2 = -2K\left(\frac{\cos 2\gamma t}{4\gamma^2} + \frac{\sin 2\gamma t}{2\gamma} \cdot t\right).$$

Hence for the complete values of C_1 and C_2 we have

$$\begin{aligned} C_1 = {} & \frac{L}{2\gamma}\sin 2\gamma t - \frac{L'}{2\gamma}\cos 2\gamma t \\ & - \frac{M-N}{4(\gamma-n)}\sin 2(\overline{\gamma-n}\,t-\lambda) - \frac{M+N}{4(\gamma+n)}\sin 2(\overline{\gamma+n}\,t+\lambda) \\ & - \frac{M'-N'}{4(\gamma-n')}\sin 2(\overline{\gamma-n'}\,t-\lambda') - \frac{M'+N'}{4(\gamma+n')}\sin 2(\overline{\gamma+n'}\,t+\lambda') \\ & - 2K.\left(\frac{\sin 2\gamma t}{4\gamma^2} - \frac{\cos 2\gamma t}{2\gamma}t\right) + c_3; \\ C_2 = {} & \frac{L}{2\gamma}\cos 2\gamma t + \frac{L'}{2\gamma}\sin 2\gamma t \\ & - \frac{M+N}{4(\gamma+n)}\cos 2(\overline{\gamma+n}\,t+\lambda) - \frac{M-N}{4(\gamma-n)}\cos 2(\overline{\gamma-n}\,t-\lambda) \\ & - \frac{M'+N'}{4(\gamma+n')}\cos 2(\overline{\gamma+n'}\,t+\lambda') - \frac{M'-N'}{4(\gamma-n')}\cos 2(\overline{\gamma-n'}\,t-\lambda') \\ & - 2K\left(\frac{\cos 2\gamma t}{4\gamma^2} + \frac{\sin 2\gamma t}{2\gamma}\right) + c_4, \end{aligned}$$

where c_3 and c_4 are arbitrary constants.

Substituting these values in equations (C.), we obtain after reduction,

$$\left.\begin{array}{ll} \left.\begin{array}{l} x = -\dfrac{L'}{2\gamma} + \dfrac{nM-\gamma N}{2(\gamma^2-n^2)}\sin 2(nt+\lambda) + \dfrac{n'M'-\gamma N'}{2(\gamma^2-n'^2)}\sin 2(n't+\lambda') \\ \qquad + c_3\cos 2\gamma t - c_4 \sin 2\gamma t + \dfrac{K}{\gamma}\cdot t; \end{array}\right\} & \quad . \quad . \quad (7.) \\ \left.\begin{array}{l} y = \dfrac{L}{2\gamma} - \dfrac{K}{2\gamma^2} - \dfrac{\gamma M - nN}{2(\gamma^2-n^2)}\cos 2(nt+\lambda) - \dfrac{\gamma M'-n'N'}{2(\gamma^2-n'^2)}\cos 2(n't+\lambda') \\ \qquad + c_3\sin 2\gamma t + c_4\cos 2\gamma t. \end{array}\right\} & \quad (8.) \end{array}\right\} (D.)$$

To determine the arbitrary constants c_3 and c_4, we have $x = 0$ and $y = 0$ when $t = 0$, which gives

$$\begin{aligned} c_3 &= -\frac{nM-\gamma N}{2(\gamma^2-n^2)}\sin 2\lambda - \frac{n'M'-\gamma N'}{2(\gamma^2-n'^2)}\sin 2\lambda' + \frac{L'}{2\gamma}, \\ c_4 &= \quad \frac{\gamma M-nN}{2(\gamma^2-n^2)}\cos 2\lambda + \frac{\gamma M'-n'N'}{2(\gamma^2-n'^2)}\cos 2\lambda' - \left(\frac{L}{2\gamma} - \frac{K}{2\gamma^2}\right). \end{aligned}$$

Equations (5) (6) (7) and (8) are the four integrals of our four differential equations.

We have now to express the coefficients of equations (D.) in terms of A, B, B′, D, and D′.

$$-\frac{L'}{2\gamma} = \frac{\gamma_2}{\gamma}\left(\frac{B}{2n}\sin 2\lambda + \frac{B'}{2n'}\sin 2\lambda'\right);$$

$$\frac{L}{2\gamma} - \frac{K}{2\gamma^2} = \frac{\gamma_2}{\gamma}\left(\frac{D}{2n}\cos 2\lambda + \frac{D'}{2n'}\cos 2\lambda'\right) + \frac{1}{\gamma}\frac{A}{2} - \frac{\gamma_2}{\gamma^2}\frac{A}{2}$$

$$= \frac{\gamma_2}{\gamma}\left(\frac{D}{2n}\cos 2\lambda + \frac{D'}{2n'}\cos 2\lambda'\right) + \frac{\gamma_1}{\gamma^2}\cdot\frac{A}{2}.$$

$$n M - \gamma N = n B - \gamma_2 D - \gamma D - \gamma\frac{\gamma_2}{n}B$$

$$= \left\{-\frac{\gamma^2 - n^2}{n}\frac{B}{D} + \gamma_1\left(\frac{\gamma}{n}\frac{B}{D} - 1\right)\right\}D;$$

$$\therefore \frac{n M - \gamma N}{2(\gamma^2 - n^2)} = \left\{-\frac{1}{n}\frac{B}{D} + \frac{\gamma_1}{\gamma^2 - n^2}\left(\frac{\gamma}{n}\frac{B}{D} - 1\right)\right\}\frac{D}{2}.$$

Similarly,

$$\frac{n' M' - \gamma N'}{2(\gamma^2 - n'^2)} = \left\{-\frac{1}{n'}\frac{B'}{D'} + \frac{\gamma_1}{\gamma^2 - n'^2}\left(\frac{\gamma}{n'}\frac{B'}{D'} - 1\right)\right\}\frac{D'}{2}.$$

$$\gamma M - n N = \gamma B + \gamma\frac{\gamma_2}{n}D - n D - \gamma_2 B$$

$$= \left\{\frac{\gamma^2 - n^2}{n} - \gamma_1\left(\frac{\gamma}{n} - \frac{B}{D}\right)\right\}D;$$

$$\therefore -\frac{\gamma M - n N}{2(\gamma^2 - n^2)} = -\left\{\frac{1}{n} - \frac{\gamma_1}{\gamma^2 - n^2}\left(\frac{\gamma}{n} - \frac{B}{D}\right)\right\}\frac{D}{2}.$$

Similarly,

$$-\frac{\gamma M' - n' N'}{2(\gamma^2 - n'^2)} = -\left\{\frac{1}{n'} - \frac{\gamma_1}{\gamma^2 - n'^2}\left(\frac{\gamma}{n'} - \frac{B'}{D'}\right)\right\}\frac{D'}{2}.$$

$$\frac{K}{\gamma} = \frac{\gamma_2}{\gamma}\cdot A.$$

$$\begin{aligned} c_3 = &\left\{\frac{\gamma_1}{\gamma}\frac{B}{D}\frac{1}{n} - \frac{\gamma_1}{\gamma^2 - n^2}\left(\frac{\gamma}{n}\frac{B}{D} - 1\right)\right\}\frac{D}{2}\sin 2\lambda \\ &+ \left\{\frac{\gamma_1}{\gamma}\cdot\frac{B'}{D'}\frac{1}{n'} - \frac{\gamma_1}{\gamma^2 - n'^2}\left(\frac{\gamma}{n'}\frac{B}{D} - 1\right)\right\}\frac{D'}{2}\sin 2\lambda'; \end{aligned}$$

$$\begin{aligned} c_4 = &\left\{\frac{\gamma_1}{\gamma}\frac{1}{n} - \frac{\gamma_1}{\gamma^2 - n^2}\left(\frac{\gamma}{n} - \frac{B}{D}\right)\right\}\frac{D}{2}\cos 2\lambda \\ &+ \left\{\frac{\gamma_1}{\gamma}\frac{1}{n'} - \frac{\gamma_1}{\gamma^2 - n^2}\left(\frac{\gamma}{n'} - \frac{B'}{D'}\right)\right\}\frac{D'}{2}\cos 2\lambda' - \frac{\gamma_1}{\gamma^2}\frac{A}{2}. \end{aligned}$$

Hence we obtain by substitution,

$$\left.\begin{aligned} x = &\left\{-\frac{B}{2n} + \frac{\gamma_1}{\gamma^2 - n^2}\left(\frac{\gamma}{n}\frac{B}{D} - 1\right)\frac{D}{2}\right\}\sin 2(nt + \lambda) \\ &+ \left\{-\frac{B'}{2n'} + \frac{\gamma_1}{\gamma^2 - n'^2}\left(\frac{\gamma}{n'}\frac{B'}{D'} - 1\right)\frac{D'}{2}\right\}\sin 2(n't + \lambda') \end{aligned}\right] \qquad \text{(E.)}$$

$$+ \left\{\begin{aligned} &\frac{\gamma_1}{\gamma}\cdot\frac{B}{2n}\sin 2\lambda - \frac{\gamma_1}{\gamma^2 - n^2}\left(\frac{\gamma}{n}\frac{B}{D} - 1\right)\frac{D}{2}\sin 2\lambda \\ &+ \frac{\gamma_1}{\gamma}\cdot\frac{B'}{2n'}\sin 2\lambda' - \frac{\gamma_1}{\gamma^2 - n'^2}\left(\frac{\gamma}{n'}\frac{B'}{D'} - 1\right)\frac{D'}{2}\sin 2\lambda' \end{aligned}\right\}\cos 2\gamma t$$

$$
\begin{aligned}
&-\left\{\begin{array}{l}
\quad \frac{\gamma_1}{\gamma}\cdot\frac{D}{2n}\cdot\cos 2\lambda - \frac{\gamma_1}{\gamma^2-n^2}\left(\frac{\gamma}{n}-\frac{B}{D}\right)\frac{D}{2}\cos 2\lambda \\
+\frac{\gamma_1}{\gamma}\cdot\frac{D'}{2n'}\cos 2\lambda' - \frac{\gamma_1}{\gamma^2-n'^2}\left(\frac{\gamma}{n'}-\frac{B'}{D'}\right)\frac{D'}{2}\cos 2\lambda' - \frac{\gamma_1}{\gamma^2}\frac{A}{2}
\end{array}\right\}\sin 2\gamma t \\
&+\frac{\gamma_2}{\gamma}A t + \frac{\gamma_2}{\gamma}\left(\frac{B}{2n}\sin 2\lambda + \frac{B'}{2n'}\sin 2\lambda'\right);
\end{aligned}
$$

$$
\begin{aligned}
y = &-\left\{\frac{D}{2n} - \frac{\gamma_1}{\gamma^2-n^2}\left(\frac{\gamma}{n}-\frac{B}{D}\right)\frac{D}{2}\right\}\cos 2(nt+\lambda) \\
&-\left\{\frac{D'}{2n'} - \frac{\gamma_1}{\gamma^2-n'^2}\left(\frac{\gamma}{n'}-\frac{B'}{D'}\right)\frac{D'}{2}\right\}\cos 2(n't+\lambda') \\
&+\left\{\begin{array}{l}
\quad \frac{\gamma_1}{\gamma}\frac{B}{2n}\sin 2\lambda - \frac{\gamma_1}{\gamma^2-n^2}\left(\frac{\gamma}{n}\frac{B}{D}-1\right)\frac{D}{2}\sin 2\lambda \\
+\frac{\gamma_1}{\gamma}\frac{B'}{2n'}\sin 2\lambda' - \frac{\gamma_1}{\gamma^2-n'^2}\left(\frac{\gamma}{n'}\frac{B'}{D'}-1\right)\frac{D'}{2}\sin 2\lambda'
\end{array}\right\}\sin 2\gamma t \qquad \text{(F.)} \\
&+\left\{\begin{array}{l}
\quad \frac{\gamma_1}{\gamma}\frac{D}{2n}\cos 2\lambda - \frac{\gamma_1}{\gamma^2-n^2}\left(\frac{\gamma}{n}-\frac{B}{D}\right)\frac{D}{2}\cos 2\lambda \\
+\frac{\gamma_1}{\gamma}\frac{D'}{2n'}\cos 2\lambda' - \frac{\gamma_1}{\gamma^2-n'^2}\left(\frac{\gamma}{n'}-\frac{B'}{D'}\right)\frac{D'}{2}\cos 2\lambda' - \frac{\gamma_1}{\gamma^2}\cdot\frac{A}{2}
\end{array}\right\}\cos 2\gamma t \\
&+\frac{\gamma_2}{\gamma}\left(\frac{D}{2n}\cos 2\lambda + \frac{D'}{2n'}\cos 2\lambda'\right) + \frac{\gamma_1}{\gamma^2}\cdot\frac{A}{2}.
\end{aligned}
$$

From equations (5) and (6.) we have

$$
x' = -\frac{\gamma_2}{\gamma_1}\frac{B}{2n}\sin 2(nt+\lambda) - \frac{\gamma_2}{\gamma_1}\frac{D'}{2n'}\sin 2(n't+\lambda') + \frac{\gamma_2}{\gamma_1}A t + \frac{c_1}{\gamma_1} - \frac{\gamma_2}{\gamma_1}x,
$$

$$
y' = -\frac{\gamma_2}{\gamma_1}\frac{D}{2n}\cos 2(nt+\lambda) - \frac{\gamma_2}{\gamma_1}\frac{D'}{2n'}\cos 2(n't+\lambda') + \frac{c_2}{\gamma_1} - \frac{\gamma_2}{\gamma_1}y;
$$

and substituting in these expressions the above values of x and y, we have

$$
\begin{aligned}
x' = &-\frac{\gamma_2}{\gamma^2-n^2}\left(\frac{\gamma}{n}\frac{B}{D}-1\right)\frac{D}{2}\sin 2(nt+\lambda) \\
&-\frac{\gamma_2}{\gamma^2-n'^2}\left(\frac{\gamma}{n'}\frac{B'}{D'}-1\right)\frac{D'}{2}\sin 2(n't+\lambda') \\
&-\left\{\begin{array}{l}
\quad \frac{\gamma_2}{\gamma}\frac{B}{2n}\sin 2\lambda - \frac{\gamma_2}{\gamma^2-n^2}\left(\frac{\gamma}{n}\frac{B}{D}-1\right)\frac{D}{2}\sin 2\lambda \\
+\frac{\gamma_2}{\gamma}\frac{B'}{2n'}\sin 2\lambda' - \frac{\gamma_2}{\gamma^2-n'^2}\left(\frac{\gamma}{n'}\cdot\frac{B'}{D'}-1\right)\frac{D'}{2}\sin 2\lambda'
\end{array}\right\}\cos 2\gamma t \qquad \text{(G.)} \\
&+\left\{\begin{array}{l}
\quad \frac{\gamma_2}{\gamma}\frac{D}{2n}\cos 2\lambda - \frac{\gamma_2}{\gamma^2-n^2}\left(\frac{\gamma}{n}-\frac{B}{D}\right)\frac{D}{2}\cos 2\lambda \\
+\frac{\gamma^2}{\gamma}\frac{D'}{2n'}\cos 2\lambda' - \frac{\gamma_2}{\gamma^2-n'^2}\left(\frac{\gamma}{n'}-\frac{B'}{D'}\right)\frac{D'}{2}\cos 2\lambda' - \frac{\gamma_2}{\gamma^2}\frac{A}{2}
\end{array}\right\}\sin 2\gamma t \\
&+\frac{\gamma_2}{\gamma}A t + \frac{\gamma_2}{\gamma}\left(\frac{B}{2n}\sin 2\lambda + \frac{B'}{2n'}\sin 2\lambda'\right);
\end{aligned}
$$

$$
\begin{aligned}
y' = &- \frac{\gamma_2}{\gamma^2 - n^2}\left(\frac{\gamma}{n} - \frac{\mathrm{B}}{\mathrm{D}}\right)\frac{\mathrm{D}}{2}\cos 2\,(n\,t + \lambda) \\
&- \frac{\gamma_2}{\gamma^2 - n'^2}\left(\frac{\gamma}{n'} - \frac{\mathrm{B}'}{\mathrm{D}'}\right)\frac{\mathrm{D}'}{2}\cos 2\,(n'\,t + \lambda') \\
&- \left\{\begin{array}{l} \dfrac{\gamma_2}{\gamma}\dfrac{\mathrm{B}}{2\,n}\sin 2\,\lambda - \dfrac{\gamma_2}{\gamma^2 - n^2}\left(\dfrac{\gamma}{n}\dfrac{\mathrm{B}}{\mathrm{D}} - 1\right)\dfrac{\mathrm{D}}{2}\sin 2\,\lambda \\ + \dfrac{\gamma_2}{\gamma}\dfrac{\mathrm{B}'}{2\,n'}\sin 2\,\lambda' - \dfrac{\gamma_2}{\gamma^2 - n'^2}\left(\dfrac{\gamma}{n'}\dfrac{\mathrm{B}'}{\mathrm{D}'} - 1\right)\dfrac{\mathrm{D}'}{2}\sin 2\,\lambda' \end{array}\right\}\sin 2\,\gamma\,t \qquad \text{(H.)} \\
&- \left\{\begin{array}{l} \dfrac{\gamma_2}{\gamma}\dfrac{\mathrm{D}}{2\,n}\cos 2\,\lambda - \dfrac{\gamma_2}{\gamma^2 - n^2}\left(\dfrac{\gamma}{n} - \dfrac{\mathrm{B}}{\mathrm{D}}\right)\dfrac{\mathrm{D}}{2}\cos 2\,\lambda \\ + \dfrac{\gamma_2}{\gamma}\dfrac{\mathrm{D}'}{2\,n'}\cos 2\,\lambda' - \dfrac{\gamma_2}{\gamma^2 - n'^2}\left(\dfrac{\gamma}{n'} - \dfrac{\mathrm{B}'}{\mathrm{D}'}\right)\dfrac{\mathrm{D}'}{2}\cos 2\,\lambda' - \dfrac{\gamma_2}{\gamma^2}\dfrac{\mathrm{A}}{2} \end{array}\right\}\cos 2\,\gamma\,t \\
&+ \frac{\gamma_2}{\gamma}\left(\frac{\mathrm{D}}{2\,n}\cos 2\,\lambda + \frac{\mathrm{D}'}{2\,n'}\cos 2\,\lambda'\right) - \frac{\gamma_2}{\gamma^2}\frac{\mathrm{A}}{2}.
\end{aligned}
$$

We have now to determine the values of A, B, D, B′, D′, γ_1 and γ_2, for which purpose (Art. 6.) we must find the values of the following quantities:

$$
\begin{array}{l}
\mathrm{A}_1\ \mathrm{B}_1\ \mathrm{D}_1 \\
\mathrm{A}_2\ \mathrm{B}_2\ \mathrm{D}_2 \\
\mathrm{A}_3\ \mathrm{B}_3\ \mathrm{D}_3 \\
\mathrm{A}_4\ \mathrm{B}_4\ \mathrm{D}_4 \\
\gamma_1\ \gamma_2.
\end{array}
$$

§. *Determination of the Numerical Values of the Constants in equations* (E), (F), (G) *and* (H).

19. *Values of* A_1, B_1, *and* D_1.—The moment of the disturbing force of the sun communicating a rotatory motion to the earth considered as a homogeneous spheroid,

$$
\begin{aligned}
&= \frac{3\,\mu}{2\,r_1^3}\cdot\frac{4\,\pi}{15}\,a_1^2\,c_1\,(a_1^2 - c_1^2)\sin 2\,\Delta \qquad \text{(Art. 1.)} \\
&= \frac{3\,\mu}{r_1^3}\cdot\frac{4\,\pi}{15}\,a_1^5\,\varepsilon\sin 2\,\Delta\,;
\end{aligned}
$$

and the moment of the forces on the shell

$$
\begin{aligned}
&= \frac{3\,\mu}{r_1^3}\cdot\frac{4\,\pi}{15}\,\varepsilon\,(a_1^5 - a^5)\sin 2\,\Delta \\
&= \frac{3\,\mu}{r_1^3}\cdot\frac{4\,\pi}{15}\,\varepsilon\,a^5\,(q^5 - 1)\sin 2\,\Delta,
\end{aligned}
$$

where $q = \frac{a_1}{a}$, the ratio of the outer to the inner radius of the shell.

The moment of inertia of the shell

$$
= \frac{8\,\pi}{15}\,a^5\,(q^5 - 1),
$$

and therefore
$$\frac{\alpha}{\omega} = \frac{3}{2}\frac{\mu}{r_1^3\,\omega}\,\varepsilon \sin 2\,\Delta$$
$$= \frac{6\,\pi^2}{T^2\,\omega}\,\varepsilon \sin 2\,\Delta \qquad\qquad (T = \text{one year})$$
$$= \frac{3\,\pi}{\nu\,T}\,\varepsilon \sin 2\,\Delta$$

(since $T\,\omega = 2\,\pi\,\nu$, $\nu = 366{\cdot}26$), which is the same as if the spheroid were entirely solid. This gives us*

$$A_1 = \frac{3\,\pi}{\nu}\,\varepsilon \sin I \cos I \,\frac{1}{T},$$
$$B_1 = \frac{3\,\pi}{\nu}\,\varepsilon \sin I \cos I \,\frac{1}{T},$$
$$D_1 = \frac{3\,\pi}{\nu}\,\varepsilon \sin I \,\frac{1}{T},$$

where I = inclination of the ecliptic.

20. *Values of* A_2, B_2, *and* D_2.—In a similar manner we obtain

$$A_2 = \frac{3\,\pi}{\nu'(\sigma + 1)}\,\varepsilon \sin I \cos I \cos^2 i \,\frac{1}{T'},$$
$$B_2 = \frac{3\,\pi}{2\,\nu'(\sigma + 1)}\,\varepsilon \cos 2\,I \sin 2\,I \,\frac{1}{T'},$$
$$D_2 = -\frac{3\,\pi}{2\,\nu'(\sigma + 1)}\,\varepsilon \cos I \sin 2\,i \,\frac{1}{T'},$$

where i = inclination of the plane of the moon's orbit to that of the ecliptic; T' = moon's sidereal period, ν' = number of days in it = 27·32; and

$$\sigma = \frac{\text{mass of the moon}}{\text{mass of the earth}} = 70 \text{ nearly}.$$

21. *Numerical value of* A_3 B_3 and D_3.—Let the interior surface of the shell be referred to three rectangular co-ordinates $x\,y\,z$, the spheroidal axis being now that of z; and let p denote the normal pressure at the point $x\,y\,z$; ξ and ζ the angles which the normal makes with lines parallel to the axes of x and z respectively. Then if

$$X = p\,.\,\delta S \cos \xi, \qquad Z = p\,.\,\delta S \cos \zeta,$$

the moment of the normal pressures with respect to the axis of y

$$= \Sigma\,(Z\,x - X\,z)$$
$$= \Sigma\,(x \cos \zeta - z \cos \xi)\,p\,.\,\delta S$$
$$= \Sigma\left(x - z\,\frac{\cos \xi}{\cos \zeta}\right) p\,\delta S \cos \zeta.$$

But
$$\cos \zeta = \frac{1}{\sqrt{1 + \left(\frac{d\,z}{d\,x}\right)^2 + \left(\frac{d\,z}{d\,y}\right)^2}},$$

---

* Airy's Tracts, p. 210.

$$\cos\xi = \frac{-\frac{dz}{dx}}{\sqrt{1+\left(\frac{dz}{dx}\right)^2+\left(\frac{dz}{dy}\right)^2}}.$$

And in the spheroid

$$\frac{x^2+y^2}{a^2}+\frac{z^2}{c^2}=1,$$

$$\therefore -\frac{dz}{dx}=\frac{c^2}{a^2}\cdot\frac{x}{z};$$

whence by substitution, the moment about the axis of y

$$= \Sigma\left(1-\frac{c^2}{a^2}\right)xp\,.\,\delta S\cos\zeta$$

$$= 2\epsilon\,\Sigma\,xp\,.\,\delta S\cos\zeta,$$

where

$$\epsilon = \frac{a-c}{a}.$$

In the determination of p it will suffice to consider the spheroidal surface as spherical; and since the disturbing force of the sun is the only force producing the rotation we are now considering, the other forces may be here neglected. Hence if the line joining the centres of the sun and earth be taken for the axis of x', and the plane through this line and the spheroidal axis for that of $x'\,z'$,

$$X = \frac{\mu}{r_1^3}\cdot 2x',$$

$$Y = -\frac{\mu}{r_1^3}\cdot y',$$

$$Z = -\frac{\mu}{r_1^3}\cdot z';$$

and

$$p = \frac{\mu}{r_1^3}\left\{2x'\delta x' - y'\delta y' - z'\delta z'\right\};$$

$$\therefore p = \frac{\mu}{r_1^3}\left\{x'^2 - \frac{1}{2}(y'^2+z'^2) + C\right\}.$$

But considering the spheroid as approximately a sphere, and $x'\,y'\,z'$ a point on its surface,

$$y'^2+z'^2 = a^2 - x'^2,$$

$$\therefore p = \frac{1}{2}\cdot\frac{\mu}{r_1^3}(3x'^2 - a^2 + C);$$

and if the plane of $x\,z$ coincide with that of $x'\,z'$

$$x' = x\sin\Delta + z\cos\Delta,$$

Δ being the sun's north polar distance, and the spheroidal axis that of z.

$$\therefore p = \frac{3}{2}\cdot\frac{\mu}{r_1^3}\left\{x^2\sin^2\Delta + xz\sin 2\Delta + z^2\cos^2\Delta - \frac{a^2-C}{3}\right\};$$

and the moment about the axis of y

$$= 3 \frac{\mu}{r_1^3} \epsilon \iint \left\{ x^3 \sin^2 \Delta + x^2 z \sin 2 \Delta + z^2 x \cos^2 \Delta - \frac{a^2 - C}{3} x \right\} \delta S \cos \zeta.$$

Let
$$z = r \cos \theta,$$
$$y = r \sin \theta \sin \phi,$$
$$x = r \sin \theta \cos \phi,$$

θ being the angle which r makes with the axis of z, and ϕ that which the plane of θ makes with that of $x\,z$. Considering the spheroid to be approximately a sphere we may put $r = a$, and $\zeta = \theta$. Also we shall have

$$\delta S = a^2 \sin \theta \,.\, \delta \theta \, \delta \phi.$$

Hence
$$\iint x^3 \cos \zeta \,.\, \delta S = a^5 \iint \sin^4 \theta \cos \theta \cos^3 \phi \, d\theta \, d\phi,$$
$$\iint z^2 x \cos \zeta \,.\, \delta S = a^5 \iint \sin^2 \theta \cos^3 \theta \cos \phi \, d\theta \, d\phi,$$
$$\iint x \cos \zeta \,.\, \delta S = a^3 \iint \sin^2 \theta \cos \theta \cos \phi \, d\theta \, d\phi,$$
$$\iint x^2 z \cos \zeta \,.\, \delta S = a^5 \iint \sin^2 \theta \cos^2 \theta \cos^2 \phi \, d\theta \, d\phi.$$

Since $\int_0^{2\pi} \cos^{2n+1} \phi \,.\, d\phi = 0$, each of these integrals except the last will vanish between the limits $\phi = 0$ and $\phi = 2\pi$. Consequently the moment about the axis of y

$$= 3 \frac{\mu}{r_1^3} \epsilon \sin 2 \Delta \, a^5 \iint \sin^3 \theta \cos^2 \theta \cos^2 \phi \, d\theta \, d\phi \quad \left\{ \begin{matrix} \phi = 0 \text{ to } \phi = 2\pi \\ \theta = 0 \text{ to } \theta = \pi \end{matrix} \right\}$$
$$= \frac{4\pi}{5} \frac{\mu}{r_1^3} \epsilon \sin 2 \Delta \, a^5.$$

The moment of inertia of the shell $= \frac{8\pi}{15} a^5 (q^5 - 1)$. Consequently the accelerating force of rotation arising from the pressure we are now considering

$$= \frac{3}{2} \frac{\mu}{r_1^3} \frac{\epsilon}{q^5 - 1} \sin 2 \Delta;$$

or if α' be the angular velocity generated by this force in a unit of time

$$\frac{\alpha'}{\omega} = \frac{3}{2} \cdot \frac{\mu}{r_1^3 \omega} \epsilon \frac{1}{q^5 - 1} \sin 2 \Delta,$$
$$= \frac{1}{q^5 - 1} \cdot \frac{\alpha}{\omega}. \qquad \text{(Art. 19.)}$$

Hence we have
$$A_3 = \frac{A_1}{q^5 - 1},$$
$$B_3 = \frac{B_1}{q^5 - 1},$$
$$D_3 = \frac{D_1}{q^5 - 1}.$$

22. *Values of* A_4, B_4, *and* D_4.—In the same manner we obtain

$$A_4 = \frac{A_2}{q^5 - 1},$$

$$B_4 = \frac{B_2}{q^5 - 1},$$

$$D_4 = \frac{D_2}{q^5 - 1}.$$

23. *Value of* γ_1.—We have seen (Art. 5. V.) that the angular motion of the solid shell produced by the centrifugal force of the fluid will be about an axis through the centre of the spheroid, and perpendicular to the plane passing through the spheroidal axis of the shell and the axis of rotation of the fluid. Let this axis be now taken for that of y', and the axis of rotation of the fluid for that of z', and let $x'\,y'\,z'$ be the co-ordinate of any particle of the fluid; then p denoting the fluid pressure there,

$$d\,p = \left(X' - \frac{d^2 x'}{d\,t^2}\right) d\,x' + \left(Y' - \frac{d^2 y'}{d\,t^2}\right) d\,y' + \left(Z' - \frac{d^2 z'}{d\,t^2}\right) d\,z'.$$

Now the impressed forces with which we are here concerned being those only which arise from centrifugal force on the fluid, we have (referring to Art. 12, and observing that the letters which are there unaccented are accented in our present notation)

$$X' = \omega^2 x', \quad Y' = \omega^2 y', \quad Z' = 2\,\omega^2 \epsilon\,\beta\,.\,x'.$$

Also, since the motion of the fluid about the axis of y' is that produced by the accelerating force $\omega^2 \epsilon\,\beta\,.\,r$ acting perpendicularly to r (Art. 12.), we have

$$\frac{d^2 x'}{d\,t^2} = -\,\omega^2 \epsilon\,\beta\,.\,z', \qquad \frac{d^2 y'}{d\,t^2} = 0, \qquad \frac{d^2 z'}{d\,t^2} = \omega^2 \epsilon\,\beta\,.\,x'.$$

Hence we have by substitution,

$$d\,p = \omega^2 (x'\,d\,x' + y'\,d\,y') + \omega^2 \epsilon\,\beta\,(z'\,d\,x' + x'\,d\,z'),$$

$$\therefore p = \frac{\omega^2}{2}\,(x'^2 + y'^2) + 2\,\epsilon\,\beta\,x'\,z' + C.$$

The moment of this force about the axis of y'

$$= 2\,\epsilon\,\Sigma\,x\,p\,.\,\delta\,S\cos\zeta, \qquad \text{(Art. 21.)}$$

$$= \omega^2 \epsilon\,\Sigma\,x\,(x'^2 + y'^2 + 2\,\epsilon\,\beta\,x'\,z' + C)\,\delta\,S\cos\zeta.$$

In this expression we may consider $x'\,y'\,z'$ as co-ordinates of a point in the surface of a sphere, whose radius $= a$. Therefore

$$x'^2 + y'^2 = a^2 - z'^2 \text{ approximately.}$$

Also, since in our results we shall only retain terms of the order $\epsilon\,\beta$, the term $2\,\epsilon\,\beta\,x'\,z'$ may be neglected, the whole quantity under the integral sign being multiplied by ϵ. This is the term arising from the effective force on the fluid.

Hence, the moment about the axis of y

$$= \omega^2 \epsilon\,\Sigma\,x\,(a^2 + C - z'^2)\,\delta\,S\cos\zeta.$$

The spheroidal axis being the axis of z we have

$$z' = z\cos\beta - x\sin\beta,$$

$$z'^2 = z^2\cos^2\beta - z\,x\sin 2\beta + x^2\sin^2\beta;$$

and substituting the expressions for x and z in terms of the polar co-ordinates as in article 21, the above expression reduces itself as in that article to

$$\omega^2\varepsilon\sin 2\beta\,\Sigma\,z\,x^2\,\delta\,S\cos\zeta$$

$$= \omega^2\varepsilon\sin 2\beta\,a^5\iint\sin^3\theta\cos^2\theta\cos^2\varphi\,d\theta\,d\varphi$$

$$= \frac{4\pi}{15}\,\omega^2\varepsilon\sin 2\beta\,a^5.$$

Dividing this by the moment of inertia $\left(= \frac{8\pi}{15}a^5(q^5-1)\right)$ we have (if α'' be the angular velocity generated in a unit of time by the force we are now considering)

$$\alpha'' = \frac{\omega^2\varepsilon}{2(q^5-1)}\sin 2\beta,$$

and

$$\frac{\alpha''}{\omega} = \frac{\omega\varepsilon}{2(q^5-1)}\sin 2\beta;$$

and therefore (Art. 5, V.)

$$\gamma_1 = \frac{\omega\varepsilon}{2(q^5-1)}$$

$$= \frac{\pi\varepsilon}{q^5-1}\,\frac{1}{t_1},$$

since if t_1 = one day, $\omega = \frac{2\pi}{t_1}$.

24. *Value of γ_2.*—We have seen (Art. 16.) that

$$\gamma_2 = \frac{\omega\varepsilon}{2}$$

$$= \pi\varepsilon\,.\,\frac{1}{t_1}.$$

25. Substituting the values of A, B, B′, D, and D′ (Art. 6.) we obtain

$$A = \frac{q^5}{q^5-1}\cdot\frac{3\pi}{\nu}\,\varepsilon\sin I\cos I\left(\frac{1}{T} + \frac{1}{\sigma+1}\,\frac{\nu}{\nu'}\cos^2 i\,\frac{1}{T'}\right),$$

$$B = \frac{q^5}{q^5-1}\cdot\frac{3\pi}{\nu}\,\varepsilon\sin I\cos I\,\frac{1}{T},$$

$$B' = \frac{q^5}{q^5-1}\cdot\frac{3\pi}{2\nu'(\sigma+1)}\,\varepsilon\cos 2I\sin 2i\,\frac{1}{T'},$$

$$D = \frac{q^5}{q^5-1}\cdot\frac{3\pi}{\nu}\,\varepsilon\sin I\,\frac{1}{T},$$

$$D' = -\frac{q^5}{q^5-1}\cdot\frac{3\pi}{2\nu'(\sigma+1)}\,\varepsilon\cos I\sin 2i\,\frac{1}{T'}.$$

These give us

$$\frac{\mathrm{B}}{\mathrm{D}} = \cos \mathrm{I},$$

$$\frac{\mathrm{B}'}{\mathrm{D}'} = -\frac{\cos 2\,\mathrm{I}}{\cos \mathrm{I}},$$

$$n = \frac{2\,\pi}{\mathrm{T}}, \qquad n' = \frac{\pi}{\tau}, \qquad \text{(Art. 5, I. and II.)}$$

$$\gamma_1 = \frac{\pi\,\epsilon}{q^5 - 1}\,\frac{1}{t_1}, \qquad \text{(Art. 23.)}$$

$$\gamma_2 = \pi\,\epsilon\,\frac{1}{t_1}, \qquad \text{(Art. 24.)}$$

$$\therefore \gamma = \gamma_1 + \gamma_2 = \frac{q^5}{q^5 - 1}\,\pi\,\epsilon\,\frac{1}{t_1}.$$

For the convenience of reference we may also put down the following ratios:

$$\frac{n}{\gamma} = 2\,\frac{q^5 - 1}{q^5}\cdot\frac{t_1}{\epsilon\,\mathrm{T}} = 2\,\frac{q^5 - 1}{q^5}\cdot\frac{1}{\nu\,\epsilon}, \qquad \left(\text{since } \frac{\mathrm{T}}{t_1} = \nu\right)$$

$$\frac{n'}{\gamma} = \frac{q^5 - 1}{q^5}\cdot\frac{t_1}{\epsilon} = \frac{q^5 - 1}{q^5}\cdot\frac{\mathrm{T}}{\tau}\cdot\frac{1}{\nu\,\epsilon},$$

$$\frac{\gamma_1}{\gamma} = \frac{1}{q^5}, \qquad \frac{\gamma_2}{\gamma} = \frac{q^5 - 1}{q^5},$$

$$\frac{\gamma_1}{\gamma^2} = \frac{1}{q^5}\cdot\frac{q^5 - 1}{q^5}\cdot\frac{t_1}{\pi\,\epsilon},$$

$$\nu = 366{\cdot}26, \qquad \frac{\tau}{\mathrm{T}} = 18{\cdot}6,$$

$$\nu' = 27{\cdot}32, \qquad \sigma = 70.$$

Also, taking the ellipticity which the earth would have had if it had been originally fluid and homogeneous, we have $\epsilon = {\cdot}004$ nearly; which also gives

$$\frac{1}{\nu\,\epsilon} = {\cdot}68.$$

Hence it appears that $\frac{n'}{\gamma}$ can never exceed $\frac{{\cdot}88}{18{\cdot}6}$ or ·047, a small quantity; but $\frac{n}{\gamma}$ may be greater than, equal to, or less than unity, according to the value of q, or the thickness of the earth's crust.

§. *Final Equations, giving the Numerical Values of x and y.*

We may now proceed to the substitution of the values found in the last section in the coefficients of equations E, F, G, and H (Art. 18.). I shall begin with equation E.

26. The coefficients of $\sin 2\,(n\,t + \lambda)$ is

$$\left\{-\frac{\mathrm{B}}{2\,n} + \frac{\gamma_1}{\gamma^2}\,\frac{1}{1 - \left(\frac{n}{\gamma}\right)^2}\left(\frac{\gamma}{n}\,\frac{\mathrm{B}}{\mathrm{D}} - 1\right)\frac{\mathrm{D}}{2}.\right.$$

Its value depends on that of $\frac{n}{\gamma}$, which may either be much less than unity, equal to unity, or greater than unity, according to the value of the ratio (q) which the outer bears to the inner radius of the shell (Art. 25.).

1. Let the shell be thin, or q nearly $= 1$; then will $\frac{n}{\gamma}$ be small (Art. 25.). Consequently the coefficient becomes

$$-\left(1-\frac{\gamma_1}{\gamma}\right)\frac{B}{2n}$$

$$=-\frac{\gamma_2}{\gamma}\cdot\frac{B}{2n}$$

$$=-\frac{q^5-1}{q^5}\cdot\frac{B}{2n}$$

$$=-\frac{3e}{4\nu}\sin I\cos I.$$

2. Let the thickness of the shell be such that $\frac{n}{\gamma}=1$ nearly. Then (Art. 25.)

$$2\frac{q^5-1}{q^5}\frac{1}{\nu e}=1 \text{ nearly,}$$

$$q=\left(\frac{2}{2-\nu e}\right)^{\frac{1}{5}} \text{ nearly,}$$

and

$$=(3{\cdot}71)^{\frac{1}{5}}=1{\cdot}3 \text{ nearly,}$$

which determines the corresponding value of the ratio of the inner and outer radii of the shell. I shall reserve this case for a distinct consideration in the sequel.

3. Let $\frac{n}{\gamma}$ be greater than unity. If the shell be so thick that q becomes considerable, γ_1 will become small, and the coefficient will become

$$-\frac{B}{2n}$$

$$=-\frac{3e}{4\nu}\sin I\cos I$$

(q being considerably greater than unity). This value is identical with that found in the former case.

The coefficient of $\sin 2(n't+\lambda')$ $\left(\text{since } \frac{n'}{\gamma} \text{ is always small}\right)$ becomes

$$-\frac{\gamma_2}{\gamma}\frac{B'}{2n'}=-\frac{q^5-1}{q^5}\cdot\frac{B'}{2n'}$$

$$=-\frac{3}{4}\frac{e}{\sqrt{(\sigma+1)}}\cdot\frac{\tau}{T'}\cos 2\,I\sin 2\,i.$$

The coefficient of $\cos 2\gamma t$ consists of two parts, of which the latter $\left(\text{since } \frac{n'}{\gamma} \text{ is small}\right)$

reduces itself to

$$\frac{\gamma_1}{\gamma^3} \cdot \frac{D'}{2} \sin 2\,\lambda' ;$$

and in like manner, when $\frac{n}{\gamma}$ is small, (or the thickness of the shell small) the first part becomes

$$\frac{\gamma_1}{\gamma^3} \cdot \frac{D}{2} \sin 2\,\lambda,$$

and the whole coefficient becomes

$$= \frac{1}{q^5} \cdot \frac{3}{2} \left\{ \frac{\sin I}{\nu^2} \cdot \sin 2\,\lambda - \frac{\sin I \sin 2\,i}{2\,\nu'^2\,(\sigma + 1)} \sin 2\,\lambda' \right\}.$$

The coefficient of $\sin 2\,\gamma\,t$ becomes $\left(\text{if } \frac{n}{\gamma} \text{ be small}\right)$

$$- \left\{ \frac{\gamma_1}{\gamma^3} \cdot \frac{B}{2} \cos 2\,\lambda + \frac{\gamma_1}{\gamma^3} \frac{B'}{2} \cos 2\,\lambda' - \frac{\gamma_1}{\gamma^3} \frac{A}{2} \right\}$$

$$= - \frac{1}{q^5} \frac{3}{2} \cdot \left\{ \frac{\sin I \cos I}{\nu^2} \cdot \cos 2\,\lambda + \frac{\cos 2\,I \sin 2\,i}{2\,\nu'^2\,(\sigma + 1)} \cos 2\,\lambda' \right.$$
$$\left. - \frac{\sin I \cos I}{\nu^2} - \frac{\sin I \cos I \cos^2 i}{\nu'^2\,(\sigma + 1)} \right\}.$$

These two coefficients being affected with the factor $\frac{1}{q^5}$, are very much diminished when q becomes considerable.

The coefficient of $t = \frac{\gamma_2}{\gamma}$ A, and becomes

$$\frac{q^5 - 1}{q^5} A$$
$$= \frac{3\,\pi\,e}{\nu} \sin I \cos I \left\{ \frac{1}{T} + \frac{1}{\sigma + 1} \cdot \frac{\nu}{\nu'} \cos^2 i\, \frac{1}{T'} \right\}.$$

The constant term becomes

$$\frac{3\,e}{4\,\nu} \sin I \cos I \sin 2\,\lambda + \frac{3\,e}{4\,\nu'\,(\sigma + 1)} \cos 2\,I \sin 2\,i\, \frac{\tau}{T'} \sin 2\,\lambda'.$$

Taking the expression for y, the coefficient of $\cos 2\,(n\,t + \lambda)$ becomes, when $\frac{n}{\gamma}$ is small,

$$- \frac{\gamma_2}{\gamma} \frac{D}{2\,n}$$
$$= - \frac{3\,e}{4\,\nu} \sin I.$$

This is also true when q becomes considerable.

The coefficient of $\cos 2\,(n'\,t + \lambda')$ becomes

$$- \frac{\gamma_2}{\gamma} \cdot \frac{D'}{2\,n'}$$
$$= \frac{3\,e}{4\,\nu'\,(\sigma + 1)} \cdot \cos I \sin 2\,i\, \frac{\tau}{T'}.$$

The numerical values of the coefficients of $\sin 2\gamma t$ and $\cos 2\gamma t$ are respectively the same as those of $\cos 2\gamma t$ and $\sin 2\gamma t$ in the expression for x.

The constant term becomes

$$\frac{3\epsilon}{4\nu}\sin \mathrm{I}\cos 2\lambda - \frac{3\epsilon}{4\sqrt{(\sigma+1)}}\cos \mathrm{I}\sin 2i\frac{\tau}{\mathrm{T}'}\cos 2\lambda'$$
$$+\frac{1}{q^5}\left\{\frac{3}{2\nu^2}\sin \mathrm{I}\cos \mathrm{I} + \frac{3}{2\nu'^2(\sigma+1)}\cos^2 i\right\}.$$

27. Hence we obtain the following expressions for x and y, for any thickness of the shell for which $\frac{n}{\gamma}$ is small.

$$\begin{aligned} x = &-\frac{3\epsilon}{4\nu}\sin \mathrm{I}\cos \mathrm{I}\sin 2(nt+\lambda) - \frac{3}{4}\frac{\epsilon}{\nu'(\sigma+1)}\cdot\frac{\tau}{\mathrm{T}'}\cos 2\mathrm{I}\sin 2i\sin 2(n't+\lambda') \\ &+\frac{1}{q^5}\cdot\frac{3}{2}\left\{\frac{\sin \mathrm{I}}{\nu^2}\sin 2\lambda - \frac{\sin \mathrm{I}\sin 2i}{2\nu'^2(\sigma+1)}\sin 2\lambda'\right\}\cos 2\frac{q^5}{q^5-1}\pi\epsilon\frac{t}{t_1} \qquad \text{(I.)} \\ &-\frac{1}{q^5}\cdot\frac{3}{2}\left\{\begin{array}{l}\frac{\sin \mathrm{I}\cos \mathrm{I}}{\nu^2}\cos 2\lambda + \frac{\cos 2\mathrm{I}\sin 2i}{2\nu'^2(\sigma+1)}\cos 2\lambda' \\ -\frac{\sin \mathrm{I}\cos \mathrm{I}}{\nu^2} - \frac{\sin \mathrm{I}\cos \mathrm{I}\cos^2 i}{\nu'^2(\sigma+1)}\end{array}\right\}\sin 2\frac{q^5}{q^5-1}\pi\epsilon\frac{t}{t_1} \\ &+\frac{3\pi\epsilon}{\nu}\sin \mathrm{I}\cos \mathrm{I}\left\{\frac{t}{\mathrm{T}} + \frac{1}{\sigma+1}\cdot\frac{\nu}{\nu'}\cos^2 i\frac{t}{\mathrm{T}'}\right\} + \mathrm{C}; \end{aligned}$$

$$\begin{aligned} y = &-\frac{3\epsilon}{4\nu}\sin \mathrm{I}\cos 2(nt+\lambda) + \frac{3\epsilon}{4\nu'(\sigma+1)}\cos \mathrm{I}\sin 2i\frac{\tau}{\mathrm{T}'}\cos 2(n't+\lambda') \\ &+\frac{1}{q^5}\cdot\frac{3}{2}\left\{\frac{\sin \mathrm{I}}{\nu^2}\sin 2\lambda - \frac{\sin \mathrm{I}\sin 2i}{2\nu'^2(\sigma+1)}\sin 2\lambda'\right\}\sin 2\frac{q^5}{q^5-1}\pi\epsilon\frac{t}{t_1} \qquad \text{(K.)} \\ &+\frac{1}{q^5}\cdot\frac{3}{2}\left\{\begin{array}{l}\frac{\sin \mathrm{I}\cos \mathrm{I}}{\nu^2}\cos 2\lambda + \frac{\cos 2\mathrm{I}\sin 2i}{2\nu'^2(\sigma+1)}\cos 2\lambda' \\ -\frac{\sin \mathrm{I}\cos \mathrm{I}}{\nu^2} - \frac{\sin \mathrm{I}\cos \mathrm{I}\cos^2 i}{\nu'^2(\sigma+1)}\end{array}\right\}\cos .\, 2\frac{q^5}{q^5-1}\pi\epsilon\frac{t}{t_1} + \mathrm{C}'; \end{aligned}$$

when C and C′ are small constant terms whose values are given above.

These are the expressions for x and y when $\frac{n}{\gamma}$ is small, or the thickness of the shell comparatively small. When the thickness is such that q becomes considerably greater than unity, the terms involving sine and cosine of $2\frac{q^5}{q^5-1}\pi\epsilon\frac{t}{t_1}$ may be entirely omitted, and the expressions will then be true in this case.

28. Since the motion of the interior fluid cannot be subjected to observation, it would be useless to make the substitution of numerical values in equations (G.) and (H.) (Art. 18.). We may remark, however, that the motion of the axis of instantaneous rotation of the fluid will be exactly similar to that of the axis of the shell, and of the same order, as is easily seen by comparing the two equations just mentioned with the equations (E) and (F) of the same article.

§. *Interpretation of the Final Expressions for x and y* (Art. 27).

29. The terms in x and y which have $2\,(n\,t + \lambda)$ for their arguments are the two parts of solar nutation. They are identical with the expressions for solar nutation deduced on the hypothesis of the earth's being a homogeneous solid spheroid. It will be recollected that this excludes the particular case in which the outer and inner radii are in a certain ratio to each other (Art. 26. 2.).

The terms of which the argument is $2\,(n'\,t + \lambda')$ are the two parts of lunar nutation, which are, for any thickness of the shell, identical with the expressions deduced on the hypothesis of the earth's being entirely solid and homogeneous.

The term in x which constantly increases with t is the luni-solar precession. It is again identical with that found on the hypothesis just mentioned. We may also remark, that this agreement is independent of any approximation depending on the smallness of such quantities as $\frac{n}{\gamma}$ or $\frac{n'}{\gamma}$, and is consequently more accurately true than in the expressions for nutation.

30. The terms of which the common argument is $2\,\frac{q^5}{q^5 - 1}\,\pi\,\epsilon\,\frac{t}{t_1}$ or $2\,\gamma\,t$ indicate an inequality depending entirely on the fluidity of the interior mass. If we denote the coefficients in these terms by G and H, and neglect the other terms, we shall have

$$x = G\cos 2\,\gamma\,t - H\sin 2\,\gamma\,t,$$
$$y = G\sin 2\,\gamma\,t + H\cos 2\,\gamma\,t;$$

or

$$x = \sqrt{G^2 + H^2}\cos 2\,(\gamma\,t + K),$$
$$y = \sqrt{G^2 + H^2}\sin 2\,(\gamma\,t + K);$$

$\left(\text{where } \tan\ 2K = \frac{H}{G}\right)$ which show that x and y would thus be the coordinates of a point moving uniformly in a circle; and if R be its radius

$$R = \sqrt{G^2 + H^2};$$

and the period of revolution would $= \frac{\pi}{\gamma}$

$$= \frac{q^5 - 1}{q^5}\,\frac{t_1}{\epsilon}.$$

It appears by the expressions for G and H, that these quantities will be the greatest when q^5 is least, i. e. when the shell is very thin; but even in that case they will not rise to magnitudes greater than those of the order of the solar nutation; and when the thickness of the shell becomes considerable, and q differs considerably from unity, the inequality will become quite insensible.

There is a corresponding inequality in the motion of the axis of instantaneous rotation of the fluid, indicated by corresponding terms in x' and y'. Comparing them with the terms in x and y, we find (omitting the other terms)

$$x' = -\frac{\gamma_2}{\gamma_1} \mathrm{G} \cos 2\gamma t + \frac{\gamma_2}{\gamma_1} \mathrm{H} \,.\, \sin 2\gamma t,$$

$$y' = -\frac{\gamma_2}{\gamma_1} \mathrm{G} \sin 2\gamma t - \frac{\gamma_2}{\gamma_1} \mathrm{H} \cos 2\gamma t;$$

or

$$x' = -\frac{\gamma_2}{\gamma_1} \sqrt{\mathrm{G}^2 + \mathrm{H}^2} \cos 2(\gamma t + \mathrm{K}),$$

$$y' = -\frac{\gamma_2}{\gamma_1} \sqrt{\mathrm{G}^2 + \mathrm{H}^2} \sin 2(\gamma t + \mathrm{K}).$$

Consequently the locus of $x'\,y'$ would also be a circle described about the common origin of $x\,y$, x' and y', and having a radius $= \frac{\gamma_2}{\gamma_1}\mathrm{R}$. By this inequality, therefore, alone the points P and P′ would describe circles about the same centre in the same periodic time, with radii in the ratio of $\gamma_1 : \gamma_2$, and differing in angular position by 180°.

The motion now described is that which would obtain if no extraneous disturbing forces acted on the spheroid, and the axes of instantaneous rotation of the shell and fluid should be separated by a small angle. It is a case of rotatory motion which has not before been investigated.

31. The case which remains for our consideration is that in which $\gamma = n$ nearly (Art. 26.).

In our previous investigations we have supposed the spheroidal shell to be of a definite constant thickness, and not to increase with the time. In the case of the earth, however, in which the solidity of the shell is conceived to be due to the external refrigeration of the mass, this thickness must be constantly increasing, though the rate of increase must be excessively slow; and our results, as expressed in equations (E), (F), (G), and (H) (Art. 18.), will be true for any instantaneous value of q, or of the thickness of the shell. So far, however, as the inequalities are of appreciable magnitude, we have seen that they are independent of particular values of q, except in the case which we have now to consider.

Referring to equation (E) (Art. 18.) we find that when $\gamma - n$ is very small, we have (taking what then become the most important periodical terms)

$$\begin{aligned} x = {} & \frac{\gamma_1}{\gamma^2 - n^2}\left(\frac{\gamma}{n}\frac{\mathrm{B}}{\mathrm{D}} - 1\right)\frac{\mathrm{D}}{2}\sin 2(n t + \lambda) \\ & - \frac{\gamma_1}{\gamma^2 - n^2}\left(\frac{\gamma}{n}\frac{\mathrm{B}}{\mathrm{D}} - 1\right)\frac{\mathrm{D}}{2}\sin 2\lambda \cos 2\gamma t \\ & + \frac{\gamma_1}{\gamma^2 - n^2}\left(\frac{\gamma}{n} - \frac{\mathrm{B}}{\mathrm{D}}\right)\frac{\mathrm{D}}{2}\cos 2\lambda \sin 2\gamma t. \end{aligned}$$

Now $\frac{\gamma_1}{\gamma^2 - n^2} = \frac{\gamma_1}{\gamma + n} \cdot \frac{1}{\gamma - n} = \frac{\gamma_1}{2n} \cdot \frac{1}{\gamma - n}$ (since $\gamma = n$ nearly);

also

$$\frac{\mathrm{B}}{\mathrm{D}} = \cos \mathrm{I}.$$

Hence putting $\frac{\gamma_1}{2n}(1 - \cos \mathrm{I})\frac{\mathrm{D}}{2} = h$, we have

$$\begin{aligned} x &= -\frac{h}{\gamma - n}\sin 2(nt + \lambda) \\ &\quad + \frac{h}{\gamma - n}\left\{\sin 2\lambda \cos 2\gamma t + \cos 2\lambda \sin 2\gamma t\right\} \\ &= \frac{h}{\gamma - n}\left\{\sin 2(\gamma t + \lambda) - \sin 2(nt + \lambda)\right\} \\ &= 2h \cdot \frac{\sin 2\,\overline{(\gamma - n)\,t}}{\gamma - n}\cos 2(nt + \lambda) \text{ very nearly,} \end{aligned}$$

an expression which assumes the form $\frac{0}{0}$ when $\gamma = n$ accurately. To put it under a more convenient form, assume t' a particular value of t, such that

$$\begin{aligned} 2(\gamma - n)\,t' &= \text{some multiple of } \pi \\ &= 2m\pi; \end{aligned}$$

and let $$t = t' + t'',$$

$$\gamma - n = s,$$

and when $$t = t',$$

let $$\gamma - n = s_1.$$

For the clearer interpretation of this term, let us first suppose the thickness of the shell, and therefore γ and s, to remain constant. We have

$$\begin{aligned} x &= 2h\frac{\sin 2 s_1 (t' + t'')}{s_1}\cos 2(n\,\overline{t' + t''} + \lambda) \\ &= \frac{2h}{s_1}\sin 2 s_1 t'' \cos 2(n t'' + \mathrm{L}). \end{aligned}$$

In a similar manner we obtain

$$y = \frac{2h}{s_1}\sin 2 s_1 t'' . \sin 2(n t'' + \mathrm{L}).$$

Since s_1 is supposed very small compared with n and the product $s_1\, t''$ may be considered as nearly constant for any one year, in which time $\sin 2(n t'' + \mathrm{L})$ will pass through its period, and the solar nutation for that year will depend on

$$\frac{2h}{s_1}\sin 2 s_1 t''.$$

Consequently from the time when $t = t'$ or $t'' = 0$, this nutation will increase every year till $2 s t'' = \frac{\pi}{2}$, after which it will again decrease. We should thus have a *secular inequality* in the solar nutation, of which the whole period would be $\frac{\pi}{s_1}$, and of which the greatest value, with reference either to x or y, would be $\frac{2h}{s_1}$.

In the actual case in which γ constantly decreases, suppose that at the time t'' from the time t',

$$s = \gamma - n = s_1 - r\,t'',$$

r denoting the rate of decrease of γ, and being taken constant during a small augmentation in the thickness of the shell. Then shall we have

$$x = \frac{2\,h}{s_1 - r\,t''} \sin \overline{2\,(s_1 - r\,t'')\,t''} \,.\, \cos 2\,(n\,t + \mathrm{L}),$$

with a similar expression for y.

This expression indicates a *secular variation* in the *secular inequality* just noticed, which increases with the diminution of $s_1 - r\,t''$, or the increase of t'', till $r\,t''$ becomes greater than s_1, after which the inequality will constantly decrease again.

The determination of r would require that of the rate at which the thickness of shell may increase. We have

$$\gamma - n = s = s_1 - \frac{d\,s}{d\,t}\,t''$$

$$= s_1 - \frac{d\,\gamma}{d\,t}\,t'',$$

$$\therefore r = \frac{d\,\gamma}{d\,t}.$$

But

$$\gamma = \frac{q^5}{q^5 - 1}\,\pi\,\epsilon\,\frac{1}{t_1} \qquad \text{(Art. 25.)}$$

$$= \frac{a_1{}^5}{a_1{}^5 - a^5}\,\frac{\pi\,\epsilon}{t_1};$$

$$\frac{d\,\gamma}{d\,t} = 5\,\frac{a_1{}^5\,a^4}{(a_1{}^5 - a^5)^2}\,\frac{\pi\,\epsilon}{t_1} \cdot \frac{d\,a}{a\,t}$$

$$= 5\,\frac{q^5}{(q^5 - 1)^2}\,\frac{\pi\,\epsilon}{t_1} \cdot \frac{1}{a} \cdot \frac{d\,a}{a\,t}.$$

Let $\delta\,a$ = increase of thickness in time T (one year); then

$$\delta\,a = \frac{d\,a}{d\,t}\,\mathrm{T},$$

and

$$r = 5\,\frac{q^5}{(q^5 - 1)^2}\,\frac{\pi\,\epsilon}{\mathrm{T}\,t_1} \cdot \frac{\delta\,a}{a}$$

$$= \cdot 35\,\frac{\delta\,a}{a} \cdot \frac{1}{\mathrm{T}\,t_1}$$

by the substitution of numerical values (Art. 26. 2.). Hence r may be known when $\frac{\delta\,a}{a}$ is determined.

Substituting for γ_1 its value $\frac{\pi\,\epsilon}{q^5 - 1}\,\frac{1}{t_1}$, and for n its value $\frac{2\,\pi}{\mathrm{T}}$, we have

$$h = \frac{\nu\,\epsilon}{8} \cdot \frac{1}{q^5 - 1} \cdot (1 - \cos \mathrm{I})\,\mathrm{D}.$$

We may here, without sensible error, put for q its particular value $\left\{\left(\frac{2}{2-\nu\epsilon}\right)^{\frac{1}{5}}\text{(Art. 26.2.)}\right\}$ when $\gamma = n$. We thus obtain

$$h = \cdot 02\,\frac{\pi}{\mathrm{T}} \text{ nearly, } 1'' \text{ being the angular unit.}$$

Also (Art. 25.)

$$\begin{aligned}\gamma - n &= \frac{q^5}{q^5-1}\,\pi\,\epsilon\,\frac{1}{t_1} - \frac{2\,\pi}{\mathrm{T}},\\ &= \left(\frac{q^5}{q^5-1}\,\nu\,\epsilon - 2\right)\frac{\pi}{\mathrm{T}},\\ &= \left(\frac{q^5}{q^5-1}\,1\cdot 464 - 2\right)\frac{\pi}{\mathrm{T}}.\end{aligned}$$

Hence the greatest value of the secular inequality $= \dfrac{2\,h}{\gamma - n}$,

$$= \left(\frac{\cdot 04}{\frac{q^5}{q^5-1}\,1\cdot 464 - 2}\right)'';$$

and the whole period of the inequality $= \dfrac{\pi}{\gamma - n}$,

$$= \frac{\mathrm{T}}{\frac{q^5}{q^5-1}\,1\cdot 464 - 2}.$$

If we assign any particular value to the above expression for $\dfrac{2\,h}{\gamma - n}$, we may easily determine the corresponding value of q, and of the thickness of the solid shell, and also the period of the inequality, supposing the thickness of the shell to remain very nearly the same during such period. Thus, suppose the greatest value of the inequality to be 5″, we shall have

$$\frac{\cdot 04}{\frac{q^5}{q^5-1}\,1\cdot 464 - 2} = 5.$$

This gives

$$\frac{1}{q} = \cdot 77 \text{ nearly,}$$

or

$$a = \cdot 77\,a_1.$$

Also when $\gamma = n$ accurately, we have (if q' be the corresponding value of q)

$$\frac{q'^5}{q'^5-1}\,1\cdot 464 - 2 = 0,$$

which gives

$$\frac{1}{q'} = \cdot 768472,$$

or, if a' be the corresponding value of a,

$$a' = \cdot 768472\,.\,a_1.$$

Therefore

$$a - a' = 6 \text{ miles approximately.}$$

The period would be about 125 years.

In order that these numerical results may be approximately true, the variation of $a - a'$ during the period of 125 years must be small compared with $a - a'$. If we suppose the thickness of our shell to increase at the same rate as that of the earth's crust in the process of its solidification, this will probably be true.

Again, if we suppose the inequality to amount to about 1000″, we obtain

$$a - a' = 130 \text{ feet nearly;}$$

and the period, supposing a constant, would be about 25,000 years, i. e. in one fourth of that period the part of solar nutation dependent on the term we are discussing would pass from zero to about 1000 seconds. If, however, we suppose the solid shell of our spheroid to increase in thickness at the same rate as the crust of the earth, the difference between a and a' would possibly not remain within the value just mentioned for nearly so long as 6000 years; in which case, supposing the inequality to be zero when $a - a'$ should equal about 130 feet, it could never afterwards amount to nearly 1000 seconds; nor could it have been previously so great, because its previous values must have corresponded to values of $a - a'$ less than the above value. Our investigation, however, does not tell us whether 120 or 130 feet would be near the value of $a - a'$ the last time the secular inequality should vanish before a became $= a'$, and consequently we cannot say with certainty that 1000 seconds would be the extreme limit to which the inequality would attain. In fact, the exact determination of this limit would require the very accurate determination of a as a function of t, which cannot be known in the case of the earth's crust without an accurate knowledge of the conductive power of the matter which constitutes it. From the small value, however, of $a - a'$ and great length of the period corresponding to the maximum of 1000″ for the inequality we have been considering, it may perhaps be deemed extremely improbable that it should ever exceed that value in the case of the earth. The duration of time for which the effect of the cause we are discussing on solar nutation would be sensible to observation would be, that necessary for the thickening of the earth's crust so to increase that $a - a'$ should pass from + (6 or 8 miles) to − (6 or 8 miles), and might therefore be approximately determined if the quantity denoted by r in this article were known.

§. *Degree of Approximation in the preceding Results.*

The results at which we have arrived above rest on the hypothesis of the instantaneous planes of rotatory motion being parallel to the tangent plane to the interior surface of the shell at B′ (Art. 8. III.); and it remains for us to consider the degree of approximation to the actual motion which has been thus obtained. It will be recollected that, on this hypothesis, the centrifugal force produces a force $Z = 2\omega^2\epsilon\beta . x$ (Art. 10.), which alone is effective in producing motion in the fluid, this motion being about the axis of y, and that which, combined with the angular motion about A B′, causes the angular motion in space of this latter axis. The value of Z has been found on the hypothesis of ω being constant, or of the rotatory motion about A B′ being the same as if the sections of the inner surface of the shell were circles instead of being ellipses of small eccentricity (Art. 9.); and the pressure on the inner surface of the shell depending on the centrifugal force has been calculated on the same hypothesis. It will be necessary therefore to examine the errors thus committed.

33. Let us conceive a closed cylinder entirely filled with fluid, which revolves uniformly about the axis of the cylinder with a velocity ω, and is not acted on by any external force. If the form of the cylinder be then changed without changing its volume, so that each section perpendicular to its axis shall become an ellipse of small eccentricity instead of being circular, it is manifest from the conditions of symmetry, that the angular motion of the fluid, though no longer uniform, will still be *steady* about the same axis, as in the circular cylinder. Consequently if p' be the pressure at any point on the surface of the elliptical cylinder, v' the velocity of the fluid at that point, we shall have

$$p' = C - \frac{1}{2} v'^2;$$

and if p and v be any corresponding values of p' and v' (which may be taken for their mean values)

$$p = C - \frac{1}{2} v^2,$$

and

$$p' = p - \frac{1}{2} (v'^2 - v^2).$$

Now the quantity of fluid which passes through any section of the elliptic cylinder made by a plane through its axis, must be constant, and therefore the velocity v' must vary inversely as r'*, the radius vector of the elliptic section from its centre. Therefore, if r be the value of r' when v is that of v' (i. e. the mean value of r')

$$v'^2 = v^2 \cdot \frac{r^2}{r'^2},$$

$$= v^2 \cdot \frac{r^2}{b'^2} (1 - 2\epsilon' \cos \theta'),$$

where b' is the axis minor of the elliptic section. Also

$$r = \frac{a' + b'}{2},$$

$$= b' \left(1 + \frac{\epsilon'}{2}\right);$$

and

$$\frac{r^2}{b'^2} = 1 + \epsilon':$$

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* There can be no doubt of this hypothesis being true, at least to a sufficient degree of approximation for our immediate purpose.

whence

$$v'^2 - v^2 . = v^2 \epsilon' (1 - 2 \cos^2 \theta') ;$$

and

$$p' = p + \frac{1}{2} v^2 \epsilon' \cos 2 \theta',$$

or, putting in the small term ωr for v,

$$p' = p + \frac{\omega^2}{2} r^2 \epsilon' \cos 2 \theta'.$$

We have here taken r' and θ' as the polar coordinates of the elliptical section of the surface, but it is evident that this expression for the fluid pressure will be equally true for any point of the fluid of which r' and θ' are the co-ordinates, p and r being taken with reference to an ellipse passing through the point $(r' \theta')$ similar to the elliptic section of the cylinder, and similarly situated.

The case which presents itself in our actual problem is analogous to the one just considered, so far as regards the elliptical form of the sections made by the planes of rotatory motion, these planes being parallel to the tangent plane at B′ (fig. 2.). The common ellipticity of these sections is $\epsilon \beta$ (Art. 9.), and, therefore, in finding the effect on the shell, of the pressure arising from the centrifugal force on the fluid in article 23., we ought to have used for p the pressure as found in the last article, p', or

$$p + \frac{\omega^2}{2} r^2 \epsilon \beta \cos 2 \theta'.$$

This, however, would only introduce into p a term of the order $\epsilon \beta$, and which, therefore, may be omitted, as shown in the investigation just referred to (Art. 23.). Also, taking this expression for p' as applicable to any point of the fluid (as explained in the preceding paragraph), it is easily seen that the force Z $(= \omega^2 \epsilon \beta . x)$ will only be altered, in consequence of the ellipticity of the sections of the shell, by a quantity small compared with itself, and which may, therefore, be neglected. Our results, then, will be quite accurate to the degree of approximation to which we have proceeded, assuming the parallelism of the planes of rotatory motion to the tangent plane at B′. I shall now proceed to this point.

34. It has been shown (Art. 15.) that the angular velocity of A B′ in space $= \omega . \epsilon \beta$, and also (Art. 28.) that the angular velocity of A B is of the same order as that A B′ i. e. of the order $\epsilon \beta$. The angular motion of A B′ (Arts. 12 ..... 15.) is due entirely to the *obliquity* of the planes of rotatory motion of the fluid particles, and the above value of it is calculated on the hypothesis of these planes of rotation being parallel to the tangent plane at B′. It is easy to see, however, that if this hypothesis be not accurate, the value of γ_2 (Art. 16.) and therefore of the angular velocity of A B′ (Art. 15.) will still be quantities of the same order respectively as the calculated values which have been given, so long as the planes of rotation shall make angles with planes perpendicular to A B′ which, instead of being = to ι (Art. 8.), shall be merely of the same order of magnitude. Without assuming, therefore, the accuracy of the above

hypothesis, we may still assert that the angular velocities of A B and A B′ will necessarily be of the order $\epsilon\,\beta$.

The positions of the planes of rotatory motion will be affected by the change of position of the shell, or of A B, and also by that of A B′. It will be convenient to consider these cases separately, first, supposing A B′ fixed while A B moves, and then taking A B fixed and A B′ in motion.

In the assumption, that A B′ shall be at rest, it is meant that it shall here be considered as unaffected by the angular motion which has been investigated, and which is due to the obliquity of the planes of rotatory motion. Our first object will be to examine whether any motion will be communicated to A B′ as the direct and immediate consequence of the motion of the shell, and independently of centrifugal force in the fluid, to which the previously calculated velocity of A B′ is entirely due; also A B′ ought strictly to be considered as *the line of quiescent fluid particles*, in which sense it will not necessarily be a straight line, as we have hitherto considered it in calculating the effects of centrifugal force on the fluid. It will, therefore, be necessary to examine the degree of its deviation from rectilinearity.

Suppose the shell to be at first in the position represented by the dotted line (fig. 2.) and then to be brought into that represented by the continuous line, A B, coinciding at first with A B′. Then while A B moves through the angle B′ A B (β), the normal motion (N N″) at any point (N) cannot exceed a quantity of the order $\epsilon\,\beta$, as is easily shown*. Also it is evident that (considering only the velocity due to the displacement of the shell) the ratio

$$\frac{\text{vel. of fluid particle at N}}{\text{vel. of point B of the shell}}$$

must be a quantity of the same order as $\frac{\mathrm{N\,N''}}{\mathrm{B'\,B}}$, i. e. of the order ϵ; and it is easily seen that for any particle in the interior of the fluid the motion cannot exceed a quantity of that order. Also the conditions of symmetry will evidently require that the particle at A should remain at rest.

If the spheroidal axis, instead of moving from A B′ to A B, move from A B to A B, the same conclusion respecting the ratio of the velocity of any fluid particle to the velocity of B will still be true, as is easily seen.

Let v_1 be the velocity of B, v that of a particle Q, from the cause we are considering, the distance of Q from A being r, and its distance from the axis A B′ $= \varrho$. Since v will be of the order $\epsilon\, v_1$ let $v = k\,\epsilon\,\frac{r}{a}\,v_1$, where k is a numerical quantity, the value of which may depend on the position of the particle. Also the velocity of Q from the motion of rotation round A B′ $= \omega\,\varrho$. Consequently, if Q be so situated that these velocities are impressed upon it along the same line and in opposite directions, the whole velocity of Q will

---

* N ν must manifestly vanish with ϵ as well as with β, and the expression for it must, therefore, involve some power of ϵ as a factor.

$$= k\,\epsilon\,\frac{r}{a}\,v_1 - \omega\,\varrho,$$

and if this $= 0$, Q will be a point in the line of quiescent particles. This gives us

$$\frac{\varrho}{r} = \frac{k\,\epsilon}{\omega} \cdot \frac{v_1}{a}:$$

$\frac{v_1}{a}$ is the angular velocity in space of A B, and is, therefore, of the order $\epsilon\,\beta$, and $= k'\,\epsilon\,\beta$ (suppose). Consequently, the angular deviation of Q from A B′ $\left(\text{which} = \frac{\varrho}{r}\right)$

$$= \frac{k\,k'}{\omega} \cdot \epsilon^2\,\beta,$$

a quantity extremely small compared with β. A B′ may, therefore, be considered as a straight line, to the required degree of approximation. Also the angular velocity of any point in A B′ due to the cause here considered, is of the order $\epsilon^2\,\beta$; it may, therefore, be neglected in comparison with the angular velocity ($\omega\,\epsilon\,\beta$) of A B′ previously determined (Art. 15.).

35. We may now proceed to consider the positions of the planes of rotation of different particles of the fluid when B does not coincide with B′. It has been shown (Art. 8. III.) that the instantaneous positions of these planes must approximate more or less to parallelism with the tangent plane at B′. This approximation, however, may be different for different fluid particles, in which case it will manifestly be most accurate for particles nearest to B′ and b', and less so for those nearer the plane of the equator. In considering, therefore, the degree of approximation it may be convenient to refer to a *mean plane* of rotation, or an imaginary plane whose inclination is the mean of the inclinations of all the planes of rotation of different particles.

As B moves about B′ the tangent plane at B′ will move from one position to another, revolving about its ultimate intersection with the consecutive position, as an axis of instantaneous rotation, with a certain angular velocity. If B moved uniformly round B′ (as would be the case if the motion were due entirely to the centrifugal force on the fluid (Art. 30.)), the angular velocity of the tangent plane would be uniform; and since the motion of the fluid would then be steady, the angular motion of the planes of rotation would also be uniform, and the angle thus described in a unit of time by the mean plane of rotation might be taken as a measure of the whole *constraining force* (arising from the reaction of the solid shell on the fluid) which produces this particular motion. In the actual case the motion of B will not be uniform; but since the *variation* of its motion will be extremely slow, the propositions just enunciated will still be approximately true for any comparatively limited period, and we may still take as a measure of the instantaneous *constraining force*, the angle actually described by the mean plane of rotation, in the manner above explained, in a unit of time.

Now if A B should move from A B' through an angle β, it is easy to show that the tangent plane at B' must move through an angle of the order $\epsilon\,\beta$; and it is easily seen likewise that if A B move in any other direction, as from A B to A $\mathfrak{B}$ through an angle β', the angle moved through by the tangent plane at B' will be of the order $\epsilon\,\beta'$. In every case, therefore,

$$\begin{aligned}\text{ang. vel. of the tang. plane} &= k'\,\epsilon\,.\,\text{ang. vel. of A B}\\ &= k\,\epsilon^2\,\beta\end{aligned}$$

where k is some finite numerical quantity. Consequently, since the angle described by the mean plane of rotation in a unit of time cannot be greater than this, that angle, and therefore the instantaneous *constraining force*, must be of the order $\epsilon^2\,\beta$.

36. Let us now consider the relation between the *constraining force* and the angle which the instantaneous mean plane of rotation makes with the instantaneous tangent plane at B'. Let ι denote, as heretofore, the angle between the tangent plane and a plane perpendicular to A B', ι' that between the tangent plane and the mean plane. Now instead of the shell moving on continuously, let us conceive its motion to cease at any instant, and consider its action, when thus at rest, on the fluid mass. If we take a fluid particle near to B' and in contact with the surface, B' may be considered as the centre of its rotatory motion, provided its distance from that point be not less than a quantity of the order $\epsilon\,\beta$ (since the angular displacement of B' cannot exceed a quantity of the order $\epsilon^2\,\beta$). Consequently, if the motion of the plane should cease at any instant, as above supposed, it is manifest that the plane of motion of this particle must be immediately constrained to coincide with the tangent plane at B', i. e. the constraining force upon it must have been such as to change its plane of motion through an angle of the order ι' in a very short space of time. If we take a particle in contact with the surface, rather more remote from B', the same conclusion must be approximately true, though a somewhat greater time may be necessary to produce an equal change in the position of its plane of rotation. And similarly if we take a particle in contact with the shell at any point, for instance, between B' and N', the reaction of the surface must produce a similar effect on its plane of rotation; and moreover, it is easily seen that if the shell be supposed to remain thus at rest for a whole revolution, for example, the effect produced in that time must be of the same order of magnitude as that for particles near to B'. Precisely the same effects must take place about b' and between b' and L', whence it will necessarily follow that similar effects and of the same order of magnitude must be produced on the planes of motion of the particles constituting the interior part of the fluid intermediate to the portions N' n' and L' l' of the surface. Similar effects must also be produced on the portion of the fluid exterior to that just specified, though these effects may decrease in magnitude as the particles are situated nearer to C and c.

Hence then it follows that (taking, for the greater distinctness, one day for the unit of time) if B, and therefore the tangent plane at B', were to remain at rest for a unit of time, the *constraining force*, estimated as above described, arising from the reaction of the shell on the fluid, would be such as to cause the mean plane of rotation to move through an angle of the same order of magnitude as the instantaneous angle between that plane and the tangent plane at B', i. e. of the order ι'. But it is evident that if the tangent plane at B', instead of remaining at rest, as we have here conceived it to do, have its actual motion *from* the instantaneous mean plane, the whole effect in one day on the plane of rotation of a particle near to B' or b' must be greater than if the surface had remained at rest; and the same conclusion must also be true for particles more remote from B' or b'. Consequently the angle through which the mean plane of rotation moves in one unit of time, must, *à fortiori*, in the actual motion be of the same order as ι', i. e. the *constraining force*, estimated by this angle, must be of the order ι'. But it has been already shown (Art. 35.) that this force must be of the order $\varepsilon^2 \beta$. Consequently ι' must be of the order $\varepsilon^2 \beta$; or, since $\iota = 2 \varepsilon \beta$, the angle between the mean plane of rotation and the tangent plane at B' is a small quantity of a higher order than ι, which proves the truth of our assumption, in the previous investigations, of the coincidence of these planes to the required degree of approximation.

37. We have hitherto considered B to move while B' remains at rest; let us now consider B' to move while B remains at rest. Suppose A B' to move through an angle β' in its motion in space which has been previously investigated, B' then coming to $\mathfrak{B}'$. If the shell were spherical, the angle between the tangent planes at B' and $\mathfrak{B}'$ respectively would $= \beta'$, and in the spheroid the angle between these planes can differ from β' only by a quantity of the order $\varepsilon \beta'$. Consequently, in order that the mean plane of rotation should be always parallel to the tangent plane at the extremity of the axis of rotation of the fluid, it must move through an angle of the same order as that (β') described by that axis; whereas when A B moves through an angle β', the corresponding angular motion of the mean plane of rotation is (as we have shown) only of the order $\varepsilon \beta'$. We must examine how this angular motion of the mean plane is produced when A B' is in motion.

While the axis of instantaneous rotation in a rigid body changes its position in the body, the instantaneous planes of rotatory motion necessarily retain their perpendicularity to it, and therefore the angular motion of those planes is equal to that of the axis. Now we have shown (Art. 15.) that the change in the position of A B' is produced in a manner exactly similar to that in a solid body, so that the same cause produces simultaneously the angular motion of A B', and an equal angular motion of the planes of rotation; whence it is easily seen that the mean plane of rotatory motion when the axis has moved to $\mathfrak{B}'$, cannot, on this account alone, deviate from parallelism with the tangent plane at $\mathfrak{B}'$ by a quantity greater than of the order $\varepsilon \beta'$. Consequently the additional angular velocity of the mean plane of motion necessary to preserve it in parallelism with the tangent plane cannot exceed a quantity of the order

$$\varepsilon \,.\, \text{ang. vel. of A B}',$$

$$= \mathrm{K}\, \varepsilon^2 \beta.$$

This additional angular velocity must be produced by the *constraining force* as previously described. The force, therefore, in this case, as well as in the one previously considered, must be of the order $\epsilon^2 \beta$; whence it also follows, as before, that the angle between the instantaneous mean plane and the tangent plane at the extremity of the axis of rotatory motion of the fluid must be of the order $\epsilon^2 \beta$, a quantity to be neglected in comparison with ι.

Also, since ι' is small compared with ι in each of the above cases considered independently, it will be likewise true when the two causes act simultaneously, i. e. in the actual motion of B and B′ about each other. Hence all our previous results will be true to the required degree of approximation.

The following then are the results at which we have arrived, supposing the earth to consist of a homogeneous spheroidal shell (the ellipticities of the outer and inner surfaces being the same) filled with a fluid mass of the same uniform density as the shell.

I. The precession will be the same, whatever be the thickness of the shell, as if the whole earth were homogeneous and solid.

II. The lunar nutation will be the same as for the homogeneous spheroid to such a degree of approximation that the difference is inappreciable to observation.

III. The solar nutation will be sensibly the same as for the homogeneous spheroid, unless the thickness of the shell be very nearly of a certain value, something less than one-fourth of the earth's radius, in which case this nutation might become much greater than for the solid spheroid.

IV. In addition to the above motions of precession and nutation, the pole of the earth would have a small circular motion, depending entirely on the internal fluidity. The radius of the circle thus described would be greatest when the thickness of the shell should be least; but the inequality thus produced would not for the smallest thickness of the shell exceed a quantity of the same order as the solar nutation; and for any but the most inconsiderable thickness of the shell would be entirely inappreciable to observation.

In my next communication I propose to consider the case in which both the solid shell and the inclosed fluid mass are of variable density.

W. Hopkins.

Cambridge,
November 19, 1838.

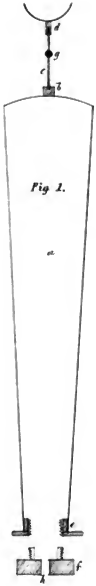

Fig. 1.

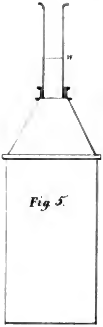

Fig. 5.

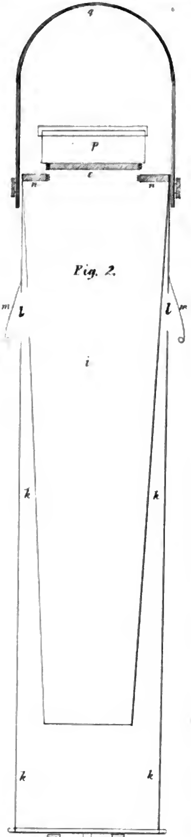

Fig. 2.

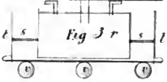

Fig. 3

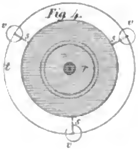

Fig. 4.

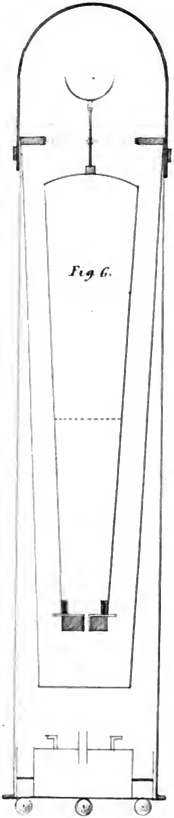

Fig. 6.

Scale ½ the real size

J. Basire sc.

XXI. *Description of a Hydropneumatic Baroscope. By* JOHN THOMAS COOPER, *Esq. Communicated by* WILLIAM THOMAS BRANDE, *Esq., F.R.S. &c.*

Received January 28,—Read February 21, 1839.

THE principle on which the instrument I am about to describe is constructed, is, *that the volume of a given quantity of air under a constant temperature, is inversely as the pressure to which it is subjected*; and the means I employ to estimate the change of volume which that quantity of air undergoes, by being subjected to differences of pressure caused by a change of elevation, are the determination of the difference of weight which a floating body is capable of sustaining in both situations. Thus, if a vessel containing a quantity of air and water be floated in water, and there be a communication between the water in the floating body and that in which it floats, it will follow, that when such an apparatus is subjected to diminished pressure, the air within the float will dilate, and cause a volume of water equal in amount to the dilatation of the air to be driven from the float; and the difference of weight which the floating body will sustain, will be the exact weight of the water expelled: if such an apparatus is subjected to an increased pressure, the air within it will contract, and consequently a quantity of water, from that in which it floats, will enter the float, and the diminished weight it is capable of sustaining will be the weight of the water which has entered the float, in consequence of the diminution of the volume of the air. It is by such means, with the instrument immediately to be described, and by the help of a very simple calculation, that I propose to determine the difference of level between any two places.

Plate X. fig. 1. represents the floating part, made of thin sheet brass, the body of which (*a*), in form the frustum of a cone, is nine inches long, two inches in diameter at one end, and one inch at the other, and capable of containing about fourteen cubic inches. In the centre of the widest end, a small stud of brass (*b*) is hard soldered, into which a brass wire (*c*) is screwed, an inch and three-eighths long, and about one twenty-fifth or one thirtieth of an inch in diameter: the other end of the wire is screwed into a brass stud in the middle of the convex side of a shallow cup (*d*), made also of brass, and as light as possible, so that it will retain its shape, and be capable of sustaining a weight of about eight hundred or one thousand grains.

At the lower and smaller end, a projecting rim (*e*) of brass is soldered, for the purpose of pouring out a portion of water from the interior with less risk of spilling; and into this end is screwed a brass plug *f*, which is required to be made of sufficient weight to sink the instrument in water with one hundred or one hundred and twenty grains in the cup at the upper part, when it contains ten cubic inches of air. To the middle of the brass wire I affix a very small quantity of red sealing-wax (*g*), melted on it at that part, to serve as an index. Through the centre of the brass plug, a small hole (*h*), about the same diameter as the wire, is to be drilled, which forms a communication between the water in the inside of the float and that in which it floats. The reason I prefer the form of an inverted frustum of an acute cone is, that the greatest portion of the weight being considerably below the centre of gravity, causes the instrument to float with greater stability, and also enables it to carry more weight in the cup, without inclining to either side.

Fig. 2. is a representation of another part of the instrument, which serves as the case for containing the float, without any chance of its becoming injured, and at the same time retains the quantity of water necessary to render it buoyant. It consists of an inner vessel (*i*) made of thin sheet copper, nearly of the same form as the float, but somewhat larger in all its dimensions, being two inches and three quarters in diameter at top, one inch and three quarters at bottom, and eleven inches and a half deep; this is surrounded by a cylindrical case (*k*) of tin plate, three inches in diameter, and fourteen inches long. About three inches from the upper part of the outer case, are four holes (*l*) three quarters of an inch in diameter, having small hoods (*m*) of tin plate soldered over them, to prevent in a great measure the wind from blowing into them. The inner copper vessel is secured to the outer cylinder of tin plate by soft solder; and both are firmly soft soldered, water-tight, to a thick circular plate of brass (*n*), in which a circular hole (*o*) is turned, a little more than two inches in diameter, for the purpose of readily allowing the float to pass through it. In the inner edge of this circular hole a female screw is cut, in order that it may be closed either by a screw plug, or by the bottom of the brass box (*p*), which serves to carry the requisite grain weights. Across the top passes a piece of thick iron or brass wire (*q*), bent into the form of a loop, answering the purpose of a handle for carriage, or for suspending it. Fig. 3. represents the elevation and plan of a small spirit lamp (*r*), requisite, when an observation is to be made, to bring the water and the float with the water and air it contains always to the same temperature. The lamp is fixed by means of three wires (*sss*) into the middle of a rim of tin plate (*t*) rather smaller in diameter than the outer cylinder, and consequently allowing it easily to slip in, and there to be secured by three small studs with bayonet-joints. An open space or interval is left between the lamp and outer rim for the free admission of air necessary for the combustion of the spirit, three small balls (*v*) serving as feet, upon which the instrument rests, so as to admit sufficient air beneath. Fig. 5. represents a vessel of copper or tin plate, capable of containing 2525 grains of distilled or rain water, having, at its open extremity, a piece of strong glass tube cemented, upon which a mark (*w*) is to be made with a file or diamond, at the place occupied by the surface of the water when it contains the above quantity, at the temperature of 62° Fahr.

The mode of adjusting the instrument I have next to describe; it is as follows:

having unscrewed the plug *h* (fig. 1.), the float is first to be filled with rain or distilled water, and then as much water is to be poured out as will fill the measure (fig. 5.) to the mark on the glass tube: ten cubic inches of water having been thus abstracted from the float, the same volume of air will supply its place. If this operation be carefully performed, the same quantity of air within two or three hundredths of a cubic inch will always be admitted, which may be proved by the instruments sustaining nearly the same weight in the cup: the plug may then be screwed into its place.

The next step of the operation is to fill the inner conical vessel (fig. 2.) with rain or distilled water, and to bring it, by means of the lamp, to the temperature at which it is determined that all the observations are to be made. Presuming that the instrument will be principally employed, for the purposes intended, in the summer season, I would recommend the adjustment and all observations to be made at the temperature of 75° or 80° FAHR. The float is next to be placed with its smaller extremity uppermost into the warmed water, that the air and water which it contains may acquire the desired temperature. I may here remark that a thermometer capable of showing at least a quarter of a degree on FAHRENHEIT'S scale ought to accompany the instrument under all circumstances, when experiments and observations are to be made. As soon as the float and its contents have acquired the requisite heat, (which may take from four to five minutes) the finger is to be placed over the small hole in the plug; then quickly withdrawing the float and replacing it inverted in the same vessel, it is adjusted for use.

In order to find the altitude by the weights to be applied in the cup, which may be considered as its scale, a long series of experiments has been made, of which the following is an abstract. The float, after being adjusted as above described, was placed in a cylindrical glass vessel containing water at 75° FAHR., the laboratory being kept at the same temperature during the time occupied by the experiments, which was generally from an hour to an hour and a half; and weights were then added to the cup, till the instrument was adjusted so that the index on the middle of the wire was coincident with the surface of the water. One hundred grains were then put into the cup, which caused the instrument to sink to the bottom of the glass vessel; and no more water was allowed to be in the glass, when the instrument had so sunk, than would stand a quarter of an inch above the mark on the wire. The cylindrical glass vessel was then placed on the plate of an air pump, and a barometer capable of reading to the $\frac{1}{1000}$th of an inch, having a cistern four inches diameter, attached to the smaller plate. The barometer was first read off and noted; a receiver was then placed over the vessel with the float, and exhaustion slowly made, until the float gradually rose from the bottom of the glass vessel, and the index on the stem coincided with the surface of the water. The barometer was then again read off and noted. A small portion of air being now let into the pump by the screw for that purpose, the float immediately sunk to the bottom. The screw, which allowed the admission of air, being made tight, the operation was repeated; and this was done a third time if the observations of the barometer differed more than two or three thousandths of an inch: a mean of the three readings was taken for the correct one. The air was now admitted into the pump, and the receiver removed, when the barometer rose to its original elevation, unless any change of atmospheric pressure had occurred during the interval, which was seldom the case; but when it did happen, a mean of the two was taken. The same operation was gone through with 200, 300, 400, 500, 600, and 700 grains, the last weight being about as much as the instrument would steadily carry on the cup without showing a tendency to overbalance. Care was taken to remove from the glass vessel an equivalent portion of water on the addition of each 100 grains, otherwise it would have risen so high in the glass as to come in contact with the bottom of the brass cup, and thereby have frustrated the experiment.

Having given a general description of the instrument and mode of adjustment, I may now refer to fig. 6, which represents the apparatus in use, and which shows the difference of the level of the water in the interior of the float, and that which is exterior to it. Although the instrument contains ten cubic inches of air when subjected only to the atmospheric pressure, it will contain somewhat less than that quantity, by being pressed upon by a column of water, equal to the difference of the two levels, which is about five or six inches.

The following Table exhibits a series of experiments made with the air-pump, and the altitudes deduced from the barometrical depressions, calculated according to the formula and tables of Mr. Bailey.

(α).	(β).	(γ).	(δ).	(ε).
grs. feet.	grs. feet.	grs. feet.	grs. feet.	grs. feet.
100 = 1062	100 = 1076	100 = 1071	100 = 1040	100 = 1040
200 = 2123	200 = 2134	200 = 2113	200 = 2012	200 = 2095
300 = 3170	300 = 3136	300 = 3140	300 = 3097	300 = 3212
400 = 4129	400 = 4124	400 = 4101	400 = 4052	400 = 4084
500 = 5087	500 = 5098	500 = 5134	500 = 4988	500 = 5026
600 = 6027	600 = 6011	600 = 5992	600 = 5859	600 = 6159
700 = 6930	700 = 6907	700 = 6895		700 = 6925

Assuming the height to be of the form $A\,G + B\,G^2$, in which G denotes the number of grains in the cup *d*, the set of observations (α) gives the following equations:

$$\begin{aligned} A + B &= 1062 \\ 2A + 4B &= 2123 \\ 3A + 9B &= 3170 \\ 4A + 16B &= 4129 \\ 5A + 25B &= 5087 \\ 6A + 36B &= 6027 \\ 7A + 49B &= 6930; \end{aligned}$$

whence by the method of least squares, we readily find $A = 1085{\cdot}27$, $B = -13{\cdot}533$.

In the same manner the coefficients A, B have likewise been determined from the other sets of observations (β), (γ), (δ), (ε), and the following are the results obtained.

	A.	B.
From the set (α)	1085·27	−13·533
(β)	1093·39	−15·207
(γ)	1086·22	−14·110
(δ)	1065·35	−14·240
(ε)	1080·77	−12·190
Means . .	1082·20	−13·856

A mean has also been taken of the number of feet corresponding to each 100 grains, and from these are found

A.	B.
1076·38	−12·687

hence we have from mean observations

	A.	B.
	1076·38	−12·687
Mean of five sets	1082·20	−13·856
Means	1079·29	13·271

The height is, therefore, expressed by the formula $1079·29\ G - 13·271\ G^2$; or if g denote the number of single grains the height is

$$\begin{aligned} h &= 10·7929\,g - ·0013271\,g^2 \\ &= g\,(10·7929 - ·0013271\,g) \end{aligned}$$

The following table, showing the factor in the parenthesis, has been constructed, retaining only three places of decimals, to the nearest figure, which are all that can be required in practice; and it is only necessary to multiply this factor by the number of grains, g, to get the height, h, in feet.

TABLE I.

−100	10·926	100	10·660	300	10·395	500	10·129	700	9·864	900	9·599
− 90	·913	110	·647	310	·382	510	·116	710	·851	910	·585
− 80	·899	120	·634	320	·368	520	·103	720	·837	920	·572
− 70	·886	130	·620	330	·355	530	·090	730	·824	930	·559
− 60	·873	140	·607	340	·342	540	·076	740	·811	940	·546
− 50	·859	150	·594	350	·328	550	·063	750	·798	950	·532
− 40	·846	160	·581	360	·315	560	·050	760	·784	960	·519
− 30	·833	170	·567	370	·302	570	·037	770	·771	970	·506
− 20	·819	180	·554	380	·289	580	·023	780	·758	980	·492
− 10	·806	190	·541	390	·275	590	10·009	790	·745	990	·479
− 0	·793	200	·528	400	·262	600	9·997	800	·731	1000	·466
0	10·793	200	10·528	400	10·262	600	9·997	800	9·731		
10	·780	210	·514	410	·249	610	·983	810	·718		
20	·766	220	·501	420	·235	620	·970	820	·705		
30	·753	230	·488	430	·222	630	·957	830	·691		
40	·740	240	·474	440	·209	640	·944	840	·678		
50	·727	250	·461	450	·196	650	·930	850	·665		
60	·713	260	·448	460	·182	660	·917	860	·652		
70	·700	270	·435	470	·169	670	·904	870	·638		
80	·687	280	·421	480	·156	680	·880	880	·625		
90	·673	290	·408	490	·143	690	·877	890	·612		
100	·660	300	·395	500	·129	700	·864	900	·599		

TABLE II.

1	1
2	3
3	4
4	5
5	7
6	8
7	9
8	11
9	12

As an example of the application of the instrument to the determination of the height of one station above another, we will suppose that, having adjusted the instrument in the manner already described, it is found that 118 grains are required to be placed in the cup, in order to sink the float so that the index on the stem shall coincide with the surface of the water, when its temperature is 75° Fahr. Having noted these particulars, returned the weights into the box, and screwed it into its place, we then proceed to the other station, the altitude of which is greater. Here the water is first brought, by means of the lamp, to the same temperature, 75°, and then weights are put into the cup, until the instrument floats at the same mark; and we will suppose it requires 274 grains to effect this:

then 274
−118
156 grains, the difference of weights supported.

In Table I. 150 gives . 10·594
In Table II. 6 gives . − 8
10·586
which multiplied by . . . 156
63·516
52930
10586
gives . . 1651·416 feet

for the difference of altitude between the two stations.

As another example, suppose that at the first station it requires 112 grains to sink the instrument to the mark, the temperature being as before 75°, and we then descend into a mine where it requires, at the same temperature, only 48 grains to bring it to the same position;

then 48
− 112
− 64 grains, the difference of weights supported:

this is minus, because the weight at the second station is less than at the first.

In Table I. − 60 gives 10·873
In Table II. − 4 gives + 5
10·878
which multiplied by 64
43512
65268
gives . . . . 696,192 feet

for the depression of the second station below the first.

As a last example, I will give the following, which will in some degree serve to show the application of the instrument to the measurement of comparatively small quantities, and the amount of reliance that may be placed on the observations made with it and also on the formula.

From the ground floor to the attics of my house it requires as nearly as possible three grains to be added to the cup to balance the instrument.

If we now take from Table I. the number answering to 0 . . . . . . 10·793
and subtract from it the number answering to 3 in Table II. 4

10·789
multiply by the difference of weights 3

the product gives the height . . . . = 32·367 feet
whereas by actual measurement it is 31 feet.

The delicacy and sensibility of the instrument are, however, such, that if it be very nicely adjusted to a tenth of a grain, it will readily show a difference of elevation of three or four feet.

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METEOROLOGICAL JOURNAL,

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AT THE APARTMENTS OF THE

ROYAL SOCIETY,

BY ORDER OF

THE PRESIDENT AND COUNCIL.

OBSERVANDA.

Height of the Cistern of the Barometer above the plinth at Waterloo Bridge....83 feet 2 inches.
———————— above the mean level of the sea ....97 feet.
Height of the receiver of the Rain Gauge above the court of Somerset House ..79 feet.
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The *monthly means* are corrected for capillarity and temperature by the Table contained in Mr. Baily's paper in *Phil. Trans.* for 1837.

METEOROLOGICAL JOURNAL FOR MAY AND JUNE, 1839.

1839.	9 o'clock, A.M. Barometer uncorrected. Flint Glass	9 o'clock, A.M. Barometer uncorrected. Crown Glass	9 o'clock, A.M. Att. Ther.	3 o'clock, P.M. Barometer uncorrected. Flint Glass.	3 o'clock, P.M. Barometer uncorrected. Crown Glass.	3 o'clock, P.M. Att. Ther.	Dew Point at 9 A.M., deg. Fahr.	Diff. of Wet and Dry Bulb Thermometer.	External Thermometers. Fahrenheit. 9 A.M.	External Thermometers. Fahrenheit. 3 P.M.	External Thermometers. Self-registering Lowest	External Thermometers. Self-registering Highest	Rain in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	REMARKS.
W 1	29.986	29.978	55.3	29.912	29.904	57.6	48	05.0	56.0	65.3	51.8	61.2		SW	A.M. Thick fog. P.M. Fine—light clouds. Evening, Cloudy.
T 2	29.918	29.944	60.9	29.918	29.910	60.2	49	07.0	60.4	66.3	51.6	64.2		NW	A.M. Cloudy—light wind. P.M. Fine—light clouds and wind. Evening, Fine and clear.
F 3	29.976	29.970	61.4	29.924	29.916	60.2	51	05.6	53.6	63.5	45.8	66.3		NNW	A.M. Lightly cloudy—light wind. P.M. Fine—light clouds and wind. Evening, Overcast.
S 4	29.832	29.824	62.8	29.728	29.720	61.3	50	06.9	59.3	66.8	51.0	61.6		E	A.M. Fine—lt. haze & wind. P.M. Fine—lt. clouds. Ev. Overcast.
⊙ 5	29.686	29.682	66.0	29.690	29.686	62.2	52	08.0	57.4	66.3	51.9	66.7		S	Fine—light clouds and wind throughout the day. Ev. Fine & clear.
M 6	29.946	29.940	62.0	29.956	29.950	61.4	51	07.5	59.7	63.7	46.0	66.7		NE	Ditto ditto. Ev. Overcast—light wind.
T 7	30.066	30.060	66.3	30.002	29.994	59.9	47	07.2	55.4	63.7	43.2	63.2		NE	Fine—nearly cloudless—light wind throughout the day. Evening, Fine and clear.
W 8	29.894	29.886	59.8	29.846	29.838	60.2	48	06.8	57.9	68.7	43.7	61.3		NE	Fine and cloudless—light brisk wind throughout the day. Evening, Overcast—thunder and lightning, with heavy rain.
T 9	29.872	29.866	60.4	29.836	29.830	58.0	47	03.2	47.2	50.7	46.3	67.4	.338	NNE	Overcast—occasional showers—brisk wind throughout the day. Evening, Heavy rain—brisk wind.
F 10	29.864	29.856	50.3	29.904	29.896	53.7	43	04.4	45.4	51.6	41.0	51.9	.422	N	A.M. Overc.—brisk wind. P.M. Cloudy—brisk wind. Ev. The same.
S 11	30.160	30.152	59.6	30.120	30.114	53.3	42	06.6	47.7	53.2	42.2	51.4	.033	N	Fine—light clouds—brisk wind throughout the day. Ev. Overcast.
⊙ 12	30.044	30.036	54.7	30.028	30.022	52.0	42	06.8	48.3	47.5	41.3	51.3		NW	A.M. Cloudy—brisk wind. P.M. Overcast—light rain—brisk wind. Evening, The same.
○ M 13	29.920	29.914	54.7	29.736	29.730	53.3	42	06.6	49.2	59.5	42.3	58.8	.068	NW	A.M. Fine—light clouds and wind. P.M. Cloudy—brisk wind. Evening, Overcast—light rain.
T 14	29.548	29.540	47.8	29.478	29.472	50.3	39	03.0	39.2	47.4	36.2	50.7	.172	NW	A.M. Overcast—light rain—brisk wind. P.M. Fine—light clouds—brisk wind. Evening, Hail and light rain.
MAY W 15	29.386	29.382	53.5	29.338	29.332	49.7	38	05.9	45.2	9.6	35.4	56.8	.016	SE	A.M. Cloudy—brisk wind. P.M. Snow, hail, & rain—brisk wind. Ev. Fine & clear.
T 16	29.556	29.550	58.2	29.526	29.518	50.8	35	06.7	46.4	53.0	33.7	63.4	.166	S	Fine—light clouds—brisk wind throughout the day. Ev. Fine & clear.
F 17	30.020	30.016	56.3	30.034	30.026	51.9	37	08.0	50.2	56.8	39.0	59.8		S	Fine—light clouds and wind throughout the day. Ev. Fine & clear.
S 18	30.124	30.118	64.9	30.068	30.060	55.2	42	08.2	53.7	58.8	42.8	69.6		S	A.M. Fine—light clouds—brisk wind. P.M. Fine—nearly cloudless—brisk wind. Evening, Overcast.
⊙ 19	30.066	30.058	53.0	30.100	30.096	57.0	48	03.7	53.7	62.0	52.2	54.3	.038	S	Overcast—deposition—light wind throughout the day. Ev. The same.
M 20	30.248	30.240	57.4	30.236	30.228	58.7	47	05.7	60.3	66.4	52.3	61.2		S	Overcast—light wind throughout the day. Evening, The same.
T 21	30.188	30.180	58.8	30.102	30.096	61.3	49	07.7	58.2	63.3	55.0	66.5		NW	Cloudy—light wind throughout the day. Ev. Fine—lt. clouds & wind.
W 22	30.004	29.996	64.3	30.046	30.040	58.6	45	07.0	50.5	49.8	47.4	65.3		NW	A.M. Dark heavy clouds, with occasional rain. P.M. Cloudy—light wind. Evening, Fine—light clouds.
T 23	30.156	30.150	61.9	30.024	30.016	57.3	43	08.4	49.8	59.0	41.6	58.0	.022	NW	Fine—light clouds—brisk wind throughout the day. Evening, Cloudy—very light rain.
F 24	29.946	29.938	58.7	30.000	29.992	56.2	43	07.0	50.3	49.8	45.2	51.7		NW	Cloudy—brisk wind throughout the day. Evening, The same.
S 25	30.120	30.114	58.9	30.138	30.132	53.8	42	07.2	47.7	50.8	41.7	52.0		N	A.M. Fine—light clouds—brisk wind. P.M. Lightly overcast—brisk wind. Evening, Fine and clear.
⊙ 26	30.216	30.210	63.9	30.184	30.180	54.4	41	05.7	48.3	58.5	40.7	60.3		NE	Fine—light clouds and wind throughout the day. Ev. Fine & clear.
M 27	30.230	30.222	59.3	30.198	30.190	56.0	43	08.1	56.3	63.8	45.0	72.2		S	Fine—nearly cloudless—light wind throughout the day. Evening, Fine—light clouds.
● T 28	30 250	30.242	62.7	30.232	30.224	58.9	50	06.8	55.5	58.4	47.2	66.0		E	Fine—light clouds and wind throughout the day. Ev. Overcast.
W 29	30.212	30.204	54.3	30.142	30.136	57.9	45	04.6	49.4	66.0	41.6	50.3		NW	A.M. Cloudy—light wind. P.M. Fine and cloudless—light wind. Evening, Fine and clear.
T 30	30.090	30.082	56.0	30.032	30.024	60.5	50	03.3	53.8	68.9	48.0	54.5		N	A.M. Overcast—brisk wind. P.M. Fine—light clouds and wind. Evening, Cloudy—light wind.
F 31	30.008	30.000	59.2	29.964	29.958	61.8	54	03.7	56.3	65.3	52.2	70.2		NW	A.M. Overcast—brisk wind. P.M. Fine—nearly cloudless.
MEAN.	29.986	29.979	58.9	29.917	29.911	56.9	45.3	06.2	52.3	58.9	45.0	60.5	Sum. 1.275		Mean Barometer corrected .... 9 A.M. 3 P.M. F 29.869 .. 29.845 C. 29.901 .. 29.878
S 1	29.964	29.956	59.0	29.910	29.902	62.2	52	05.3	55.2	68.0	48.2	55.7		N	Fine—light clouds and wind throughout the day. Evening, Cloudy.
⊙ 2	29.906	29.900	65.9	29.832	29.828	61.0	48	06.7	53.7	56.9	45.6	68.0		NE	A.M. Fine—light clouds and wind. P.M. Overcast—light wind. Evening, Overcast—light rain.
M 3	29.694	29.686	54.9	29.658	29.650	58.9	48	01.4	50.8	62.3	47.0	51.3	.227	ENE	A.M. Overcast—heavy rain during the night. P.M. Fine, with occasional showers. Evening, Overcast—light rain.
T 4	29.666	29.658	59.6	29.668	29.660	60.2	51	03.9	54.6	60.4	50.3	54.7	.069	E	Overcast—light wind throughout the day. Evening, The like.
W 5	29.826	29.830	60.9	29.856	29.850	61.2	52	06.0	59.3	67.2	53.3	61.7		N	A.M. Overcast—light breeze. P.M. Fine—light clouds. Evening, Cloudy—light rain.
T 6	29.970	29.964	75.4	29.952	29.946	64.7	55	08.6	63.9	67.4	54.2	57.9	.227	S	Fine—light clouds and wind throughout the day. Ev. Cloudy.
F 7	29.848	29.842	62.2	29.816	29.808	64.3	56	05.2	60.3	61.4	56.3	69.3	.033	E	A.M. Overcast—light steady rain. P.M. Overcast—light wind. Evening, The like.
S 8	29.898	29.892	62.9	29.864	29.856	65.5	56	07.0	63.2	68.8	55.9	63.7	.091	SE var.	A.M. Overcast—brisk wind. P.M. Fine—light clouds and wind. Evening, Cloudy.
⊙ 9	30.058	30.052	71.2	30.094	30.090	68.0	57	09.5	63 8	69.0	56.0	85.0		S	Fine—lt. clouds—brisk wind throughout the day. Ev. Fine & clear.
M 10	30.318	30.312	66.9	30.284	30.278	67.6	56	09.3	63.9	69.6	55.5	71.2		SW	Fine—light clouds and wind throughout the day. Evening, Cloudy—brisk wind.
○ T 11	30.262	30.258	70.8	30.288	30.280	67.6	59	08.4	64.7	70.3	59.4	70.0		W	Fine—light clouds—brisk wind throughout the day. Ev. The like.
W 12	30.244	30.238	74.6	30.132	30.124	69.3	59	08.7	67.3	74.3	57.8	80.7		S	Fine—light clouds, with light brisk wind throughout the day. Evening, Fine & clear.
T 13	29.944	29.938	83.3	29.910	29.902	72.7	65	09.9	72.0	71.8	62.2	78.3		NE	A.M. Fine and cloudless—light wind. P.M. Cloudless—brisk wind. Evening, Overcast—distant thunder.
JUNE F 14	29.866	29.858	65.6	29.876	29.870	65.9	57	02.9	58.2	62.0	55.3	77.6		NE	Overcast—brisk wind throughout the day. Evening, Cloudy.
S 15	29.946	29.940	63.7	30.046	30.038	64.9	58	02.8	58.4	61.6	55.7	64.4		NNW	Lightly overcast—light breeze throughout the day. Ev. Cloudy.
⊙ 16	30.268	30.262	75.6	30.264	30.260	66.9	53	08.0	61.3	70.2	49.0	65.0		NE	Fine & cloudless—lt. breeze throughout the day. Ev. Fine & clear.
M 17	30.200	30.192	66.0	30.154	30.148	67.8	58	06.8	64.2	69.3	52.5	70.3		ENE	Fine—nearly cloudless—light breeze throughout the day. Evening, Lightning, with rain.
T 18	30.054	30.050	75.2	29.962	29.954	71.3	65	07.7	70.6	77.6	61.6	83.6		E	A.M. Fine—light clouds and wind. P.M. Fine—light clouds—at ½ p. 5 o'clock, thunder with heavy rain. Ev. Cloudy—much lightning.
W 19	29 998	29.992	74.4	30.060	30.054	71.3	62	07.2	66.6	71.4	62.0	82.3	.050	S	A.M. Cloudy—lt. wind—thunder early. P.M. Fine—lt. clouds—brisk wind. Ev. Fine & clear.
T 20	30.158	30.152	78.9	30.038	30.030	71.8	62	08.0	67.8	75.7	58.5	85.0		S	A.M. Cloudy—light breeze. P.M. Fine—light clouds. Ev. Cloudy—very lt. rain.
F 21	29.818	29.814	78.6	29.822	29.816	71.2	62	08.2	68.3	70.7	61.0	76.2	.033	S	A.M. Fine—light clouds and wind—very high wind early. P.M. Clouds—brisk wind. Evening, Fine and clear.
S 22	29.644	29.640	67.8	29 460	29.452	66.9	60	07.1	63.9	61.7	59.9	73.6		S	A.M. Cloudy—high wind—very light rain early. P.M. Overcast—high wind—light rain. Evening, Fine—light clouds.
⊙ 23	29.456	29.450	72.9	29.572	29.566	67.3	57	08.0	62.8	62.0	56.8	70.0	.111	SE var.	A.M. Cloudy—very high wind. P.M. Cloudy—light rain—high wind. Ev. Dark heavy clouds—high wind, with light showers.
M 24	29.842	29.836	71.9	29.870	29.862	67.9	58	08.1	62.4	68.3	55.6	76.4	.319	S	A.M. Cloudy—light wind. P.M. Fine—light clouds. Ev. The like.
T 25	29.968	29.962	68.8	29.918	29.912	67.3	57	07.9	64.3	69.8	57.4	69.3		S	Fine—light clouds and wind throughout the day. Evening, Overcast—light wind.
● W 26	29.552	29.546	66.8	29.476	29.470	66.7	58	06.3	63.4	67.0	56.0	70.4		E	A.M. Cloudy—thunder—light rain—high wind. P.M. Continued. Evening, Overcast—light rain.
T 27	29 892	29.888	73.0	29.846	29.840	66.8	57	08.5	62.8	66.7	54.2	68.6	.083	W	Fine—lt. clouds—brisk wind throughout the day. Ev. Ovct.—lt. rain.
F 28	29.684	29.680	70.3	29.706	29.702	64.6	57	07.5	61.3	58.3	54.7	81.2	.133	S	A.M. Fine—light clouds, and with occasional rain. P.M. Overcast—light rain—brisk wind. Evening, The like.
S 29	29.956	29.950	59.9	30.038	30.032	61.7	52	04.3	53.7	59.2	50.3	54.0	.144	WNW	Overcast—light wind throughout the day. Evening, The like.
⊙ 30	30.158	30.150	60.9	30.204	30.200	59.5	46	06.7	53.3	54.0	45.8	53.8		W	Cloudy—light wind throughout the day. Evening, The like.
MEAN.	29.936	29.930	68.7	29.919	29.913	66.1	56.4	06.9	61.9	66.3	54.9	70.2	Sum. 1.520		Mean Barometer corrected .... 9 A.M. 3 P.M. F. 29.832 .. 29.823 C. 29.825 .. 29.816

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